THE SNELLIUS-EXPEDITION

IN THE EASTERN PART OF THE NETHERLANDS EAST-INDIES 1929-1930

UNDER LEADERSHIP OF
P. M. VAN RIEL
DIRECTOR OF THE OCEANOGRAPHIC AND MARITIME METEOROLOGICAL
DEPARTMENT OF THE ROYAL NETHERLANDS METEOROLOGICAL INSTITUTE

VOL. V

GEOLOGICAL RESULTS

PART 1

GEOLOGICAL INTERPRETATION OF THE BATHYMETRICAL RESULTS

BY

Dr. Ph. H. KUENEN
(GEOLOGIST OF THE EXPEDITION)

(THE BATHYMETRICAL CHARTS OF THE EXPEDITION ARE CONTAINED IN VOL II, PART 2, CHAPTER II)

1935

TO BE OBTAINED OF THE PRINTERS AND PUBLISHERS
E. J. BRILL — LEIDEN

SNELLIUS-EXPEDITIE

WETENSCHAPPELIJKE UITKOMSTEN DER SNELLIUS-EXPEDITIE

ONDER LEIDING VAN

P. M. VAN RIEL

DIRECTEUR DER AFDEELING OCEANOGRAPHIE EN MARITIEME METEOROLOGIE
VAN HET KONINKLIJK NEDERLANDSCH METEOROLOGISCH INSTITUUT

VERZAMELD IN HET OOSTELIJKE GEDEELTE VAN NEDERLANDSCH OOST-INDIË
AAN BOORD VAN H. M. WILLEBRORD SNELLIUS

ONDER COMMANDO VAN

F. PINKE

LUITENANT TER ZEE DER 1e KLASSE

1929–1930

UITGEGEVEN DOOR DE MAATSCHAPPIJ TER BEVORDERING VAN HET
NATUURKUNDIG ONDERZOEK DER NEDERLANDSCHE KOLONIËN EN
HET KONINKLIJK NEDERLANDSCH AARDRIJKSKUNDIG GENOOTSCHAP

GEDRUKT DOOR EN TE VERKRIJGEN BIJ
E. J. BRILL — LEIDEN

THE SNELLIUS-EXPEDITION

IN THE EASTERN PART OF THE NETHERLANDS EAST-INDIES 1929-1930

UNDER LEADERSHIP OF

P. M. VAN RIEL

DIRECTOR OF THE OCEANOGRAPHIC AND MARITIME METEOROLOGICAL DEPARTMENT OF THE ROYAL NETHERLANDS METEOROLOGICAL INSTITUTE

VOL. V

GEOLOGICAL RESULTS

PART 1

GEOLOGICAL INTERPRETATION OF THE BATHYMETRICAL RESULTS

BY

Dr. Ph. H. KUENEN

(GEOLOGIST OF THE EXPEDITION)

(THE BATHYMETRICAL CHARTS OF THE EXPEDITION ARE CONTAINED IN VOL II, PART 2, CHAPTER II)

1935

TO BE OBTAINED OF THE PRINTERS AND PUBLISHERS

E. J. BRILL — LEIDEN

CONTENTS

CHAPTER I

INTRODUCTION

Volume V of the Snellius Expedition will contain the reports on the marine geology. Besides Part 1, presented here and Part 2 on the geology of coral reefs, that has already been published, a third part is to be compiled on the bottom samples. The investigations dealing with the regional geology of the Netherlands East Indies are to be published separately in periodicals.

For the proper understanding of Part 1 of Volume V the bathymetrical charts of the expedition should be consulted. They are contained in Chapter II, Part 2 of Volume II (bibl. 91). This chapter may be obtained separately and the charts are also for sale unfolded.

One of the principal tasks of the Snellius Expedition was to collect as much information as possible concerning the morphology of the sea floor in the eastern half of the Netherlands East Indies.

This part of the globe is characterised by an intricate system of deep basins, divided by island arcs and shallow ridges. For the investigation of the circulation of the sea water in these basins and of its interaction with the waters coming from the Indian Ocean and from the Pacific, it is of primary importance to know the shape and depth of the basins and especially of the passages which lie between them and connect them with the open ocean. It is also essential to obtain an accurate knowledge of the shape of the basins and troughs and of the submarine ridges and plateaus in order to gain a deeper insight into the major features of the geological structure of this region, especially of the nature of the tectonic processes at work. Though the knowledge of the geology of the islands had increased by leaps and bounds in the last 30 years, the deep sea chart had hardly developed since the Siboga expedition published its results at the beginning of this century. Thus both oceanographer and geologist were united in demanding a substantial increase of our knowledge of the bathymetry of the East Indies.

Now that Behm's wonderful new invention of echo sounding had been developed into a practical method for finding the depth at any moment during the cruise, the ship for the expedition had of course to be fitted with an apparatus for echo sounding.

An oceanographical expedition, however, has so many calls upon its limited time that it cannot set out to sound an entire region systematically. Broadly speaking, the sounding had to be restricted to the investigation of the sections by which the oceanographical stations were connected. In addition, the programme included the separate investigation of a number of regions that were considered of special importance. The latter comprised a number of entrances to basins where important currents effectuate the renewal of the water in the basins, and also a number of regions that might throw light on geological questions.

As geologist of the expedition it was the task of the present author to choose which parts called for a more detailed investigation by soundings, in order to solve structural problems. Professors Escher, Molengraaff, Brouwer and Rutten kindly gave helpful criticism of my plans. The leader of

the expedition, Mr. P. M. van Riel, fitted these geological desiderata into the general plan as far as time allowed and finally decided which sections were to be sounded. Mr. van Riel, Lieutenants Pinke and Perks and the other members of the expedition gave helpful advice during the cruise, when new problems arose as to how the sounding sections could be best laid to solve these difficulties. Finally, Professor Vening Meinesz kept in touch with the expedition during his simultaneous gravimetrical investigation of the East Indies on board Hr. Ms. submarine K. XIII, exchanging bathymetrical results and pointing out a number of problems that arose from his own investigations.

The bathymetrical investigations that were undertaken mainly to solve geological problems are the following: the northern end of the Strait of Makassar for the relation of Borneo to Celebes, the region east of Tarakan on Borneo, the south end of the Mindanao Deep for the connection between Morotai and the Palao Islands, the Molukken Sea, the strait south of Soela Sanana and Manipa Strait for the connections of Boeroe with its surroundings, the strait between Lifamatola and Obi, the region south of Ceram for the continuation of the Inner Banda Arc, the region between Babar and the Tanimbar Islands for the nature of the outward bend of the Outer Banda Arc, the surroundings of Kisar, the region south of Soemba for the continuation of the Java Deep, the Toekangbesi Group for the situation of the atolls.

The new charts are entirely the result of work done by the Navy. The sounding, the reduction to true depth, the determination of the positions, the mapping, the construction of the sections were all carried out on board ship during the expedition by the naval officers and members of the crew. For the 30.000 new and 3000 old soundings this represents an enormous amount of labour. Afterwards again it was the Hydrographical Survey Dept. who drew the definite chart and checked all the data it contains. A fuller acknowledgement of individual work will be found elsewhere in the reports of the expedition (Volume II, Part. 2, Chapter II). It is my wish, however, to express with great emphasis the debt of gratitude that science in general and geology in particular owes to the untiring energy with which this great amount of work was carried out by the anonymous workers of the Navy and the great accuracy that was attained throughout. All those who in future will have recourse to the bathymetrical chart of the east part of the East Indies, whether for local details of the structure or for general problems of the development of island arcs and deep-sea troughs, should remember that it is the Hydrographical Survey Dept. of the Royal Dutch Navy which supplied the great mass of new data.

CHAPTER II

THE SOUNDINGS, THE CHART AND THE SECTIONS

1. METHODS USED ON BOARD THE „SNELLIUS".

It is not my intention to give a detailed technical description of the sounding machines and the methods used during the expedition. Those who wish to learn more concerning these subjects should consult the oceanographical volumes of the Snellius Report. A few remarks will suffice for the geologist to learn how the data were obtained.

The wire soundings were taken with a Lucas sounding machine. The bottom samples will be described in the Part on the deep-sea deposits. The number of soundings taken outside the 200 m line in the area covered by the chart was about 300. To these should be added a dozen soundings taken with the large sampler on the machine for the oceanographical series.

The bulk of the new soundings, some 30.000, were obtained by echo sounding. There were two echo sounding machines on board, an Atlas sounding machine and a Hughes sounding machine. The accuracy of the latter apparatus is greater, but as the echo could seldom be heard deeper than 1500 m and it was moreover frequently out of order, we were practically entirely dependent on the Atlas machine.

The soundings were generally taken at intervals of 10 or 15 minutes, that is about every 2 km. As soon, however, as irregularities appeared or could be expected the interval was reduced to 5 or 2½ minutes and in a few cases the soundings were taken even closer together.

An apparatus for ascertaining the direction of the echo in order to find the direction and amount of slope of the bottom failed to give satisfactory results.

2. ACCURACY OF THE SOUNDINGS.

The geologist who wishes to study the morphology of the sea bottom cannot entirely neglect the problem of the accuracy of the soundings he uses. A more detailed and technical treatment of this problem, especially from the nautical point of view, will be given elsewhere in the Snellius Reports (Volume II, Part 2) by Lieutenant F. Pinke. As Mr. Pinke's report is not yet available, the following remarks must be looked upon as preliminary. I trust, however, that they will prove sufficiently reliable for the purpose of geologists, who need only to know roughly what the order of accuracy of the soundings is. Those readers who are interested in the accuracy as a problem are referred to the report mentioned.

The accuracy of a sounding depends on three values: the amount, the position on the earth's surface and the position with relation to the neighbouring soundings.

The sources of error of the amount of the depth as given by a *wire sounding* are manifold: Slip [1])

[1]) The principal reason for believing slip over the measuring wheel to be small is the following. The reading is always more for the amount of wire hauled in than for that played out. As the wire stretches very little this must be principally due to a greater amount of slip during the playing out when the strain on the wire is in the order of $^1/_8$ of that during the hauling in. This is confirmed by the fact that the difference is smaller when we use a twisted wire

over the measuring wheel and wear of this wheel, stretching and temperature of the wire are of minor importance, if we calculate the depth from the readings of the sounding machine when hauling in the wire. By far the largest error is made when the wire does not hang absolutely taut and vertical. Drifting of the ship with relation to the surface layers can generally be fairly successfully compensated by manoeuvring, but a surface current of appreciable strength is difficult to counteract. As these currents are limited to the first few hundred meters the wire probably hangs nearer the vertical in the stagnant deeper layers, thus lessening the error, by comparison, with greater depths. Our soundings were corrected roughly, in proportion to the measured angle of the wire above water at the moment the bottom was reached. For the older soundings on the chart this precaution was probably not taken.

Although the error is not directly proportionate to the depth it is nearly so for shallower soundings and it does augment with greater depth. In most soundings of more than 1000 m the error is probably less than 1 %, the angle of the wire seldom exceeding 10°, and it will hardly ever be more than 2 % [1]). Moreover the depth is always found to be greater when the wire runs down obliquely. As long as the position is correct all wire soundings, otherwise reliable, may mark too great a depth. Greater errors than 50 m, even for the deepest soundings are probably a great exception after correction.

The sources of error of the *amount* given by an Atlas *echo sounding* are also manifold. The velocity of propagation of sound in the sea water depends on the physical and chemical composition of the water. These were determined with sufficient accuracy during the expedition to render the remaining errors from this source negligeable. There remain the mistakes made by the man at the sounding machine. With the Atlas machine the observer notes the position of a lamp revolving along a scale at the moment he hears the echo. With practice and care the mistake made should seldom exceed 30 meters. This mistake is irrespective of the absolute depth. Under unfavourable circumstances the error may increase to twice or even three times this amount and a careless observer makes errors of 100 m and more. We may safely conclude that apart from a few unfortunate cases all echo soundings are within 50—100 m of the actual (shortest) distance from ship to sea bottom. (When the bottom slopes the depth, measured vertically, is greater than this distance).

The *difference* in depth between *successive soundings* is found on the whole to contain smaller errors than the absolute depth, as systematic errors are eliminated. In how far these systematic errors are constant for each individual and perhaps for all observers is not yet known. It would be of great importance, and should be a simple matter, to investigate these systematic errors. If the revolving lamp were made to close a current when passing a movable contact on the depth scale (invisible to the sounder) and thus to produce an imitation of the echo, we could set the depth to any desired amount. The length of the contact should be variable as well as the volume of the sound. By this means it would be possible to ascertain the personal errors of the sounders before and during the cruise.

A second source of error with echo soundings occurs when the sea floor is not horizontal. The signal is returned not from the bottom vertically below the ship, but from the spot where the perpendicular from the ship meets the bottom. This point lies towards the shallower side of the slope. The error is proportionate to the depths and grows with the increase of the slope (fig. 1).

---

which is of course rougher. The friction of the measuring apparatus hardly increases with the increased strain, as the velocity is also smaller when hauling in. The friction between wire and measuring wheel, on the other hand, will be roughly proportionate to the strain on the wire. During the hauling in the strain is about 5 times as large and more constant, so that the slip will be in the order of $^1/_5$, the difference in the readings therefore about 4 × the slip during the hauling in. As the difference between the two readings is roughly proportionate to the depth and less than ½ %, the amount of slip during the hauling in must be negligeable as compared to other sources of error. The reading belonging to the hauling in was taken as indicating the length of wire played out.

[1]) Although this is only a rough estimate the following two observations may be cited as confirmation. Once a depth of 5000 m was sounded, while the wire was played out at the exceptional angle of about 30°. The ship then steamed very slowly towards the wire that was kept taut. When the angle had decreased from 30° to 10° the sampler came up out of the mud. Only 100m or 2 % had been hauled in. In another case, bottom was found at 1598m with an angle of 10°. Steaming slowly until the sampler came up at about 0° the depth reading decreased only to 1575 or 1 ½ % less. According to Maurer (bibl. 80, p. 51) Wüst calculated the actual depth to be 99.7 % for an angle of 7°, 99.4 % for 12°, 98.8 % for 17°.

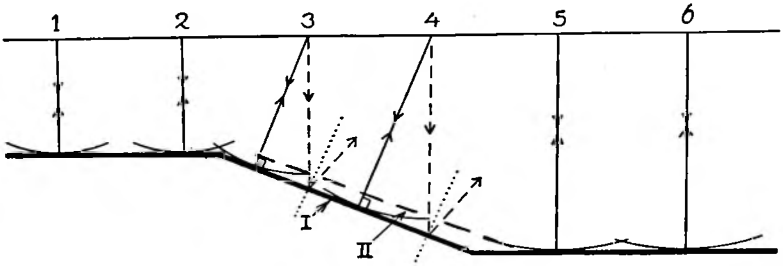


Fig. 1. Construction of the correct bottom section from a series of echo soundings. I = correct section, II = apparent section.

As the top of a ridge or the bottom of a trough are generally horizontal the correct minimum or maximum are still found. [In the case of a very narrow trough the greatest depth is not reached by an echo sounding (see fig. 11)]. The ridge, however, will appear too broad, the trough too narrow, as a result of the decrease of the slopes. In practically all cases the error is smaller than the possible error in the determination of the position of the sounding and may be ignored in the construction and interpretation of the *chart*. In the following paragraph it will be shown how the error is reduced as far as possible in the *sections*.

The *position* on a chart of a sounding is a nautical problem, but the geologist wishes to know its accuracy before drawing his conclusions. This cannot be stated in a general way, but the following remarks will help to give us some idea of the possible amount of error. Positions are obtained by astronomical determination or by bearings on points on the coast. The former were obtained with a possible error of about 1 nautical mile, the latter are more exact. Within some 10 miles of the coast, therefore, the position is reliable in the daytime, barring bad weather. Away from the coast, the accuracy is greatest close to the astronomical positions. The number of these is generally two in twenty-four hours. The log and compass help to divide the intervening space in the correct proportion to the speed. The influence, however, of irregularities in the currents, winds, etc. cannot be eliminated. Errors of several miles may thus occur. If for a given sounding greater certainty as to the accuracy is desired, the only method is to consult the ship's log to see how the position was obtained.

The large chart is drawn to a scale of 1 in 2.500.000 (1 mm = 2½ km). The true position of a sounding will therefore fall within the area occupied by the central figures of the number itself, except in very unfortunate cases.

It is obvious that echo soundings can generally be located with greater accuracy than wire soundings, because the ship sails on regularly without stopping, thus allowing of a more trustworthy interpolation between the accurately fixed points.

We have still to consider the *position* of the soundings with relation to the *neighbouring soundings*. This is of special importance when sections are to be drawn. A small change in the position of a sounding seldom alters the depth curves on the chart to an appreciable extent. The features of the submarine topography as ridges and troughs remain where they are, but are slightly deformed. With sections, however, it is a different matter. Although here also culminations and depressions are not much influenced in their position, the slopes, especially where they are steep, may undergo great alterations by relatively small movements of the positions of the soundings.

For the construction of sections, therefore, echo soundings are greatly superior to wire soundings. Thus the ship may drift unobserved almost as much as the amount of the depth between two successive wire soundings, while two successive echo soundings cannot be out of place with regard to eachother more than a small percent of their horizontal distances. The closer together the soundings are, and the deeper the water, the greater is the advantage of echo soundings over wire soundings to give us the correct slope. The most extreme case is that of vertical drops of the bottom. The existence of these can only be proved by echo sounding.

The following were the means employed to compare the various methods as regards accuracy and the general reliability of the sounding results:

Echo soundings taken simultaneously with the two echo sounding machines and together with wire soundings.

Comparisons between various people with the Atlas machine.

Depth determinations with thermometers.

The study of the successive depth readings in a sounding section. This method for gaining a general impression of the degree of accuracy of the echo soundings is by considering the sounding sections and the charts, keeping in mind that the echo sounder knows neither what the next sounding is going to be when he reads off the depth, neither what the character of the neighbouring sections is. Now if the depth at a row of sounding points is for instance: 1500, 1500, 1570, 1600, 1560, 1500, 1500, we may conclude that the difference in depth of two successive soundings (for this case) is in all probability more accurate than 100 m. When the third sounding was taken it was not known that the next sounding was going to be still more. Yet the increase in depth of less than 100 m was noticed. In the same way, at the fifth sounding the still smaller depth at the sixth could not be foreseen. In other words, if a change in depth of x meters between two soundings is led up to by a sounding of intermediate depth in between, the accuracy (with which the difference is indicated) must be greater than x. This reasoning can of course only be applied to soundings that are so close together that the changes from upwards to downwards of the bottom occur at considerably larger distances from eachother than the soundings, otherwise even very accurate soundings would show an arbitrary variation in depth. The general impression to be obtained from this way of looking at the chart is also that successive soundings give the alteration in depth with an accuracy considerably greater than 100 m.

A few examples taken from our charts are as follows:

a) 1170, 1080, 1040, 990, 960, 950, 890, 920, 1000, 1010, 950, 980, 710.

b) 550, 700, 760, 890, 920, 1000, 1060, 1030, 990, 1150, 1260.

c) 1170, 1140, 1080, 1050, 1000, 980, 970, 1030, 1080, 1130, 1160, 1190, 1140, 1070, 1050, 1030, 1000.

d) 3360, 3410, 3370, 3360, 3250, 3210, 3280, 3260, 3160, 3060, 3180, 3140, 3230, 3090, 2980.

Finally, the points at which sounding lines crossed offered an excellent opportunity for checking the combined accuracy of place and depths. On the whole these gave very satisfactory results and generally a slight change of position was sufficient to coordinate the two sections perfectly.

In fig. 2 a number of examples are given of the crossings of sounding lines. They were chosen quite arbitrarily from the scores of crossings shown on the fair sheets. Some were situated far from the shore, others represent cases where the position was obtained from occasional bearings on the neighbouring coasts. It should be remembered that during the sounding one does not know at what point the crossing has occurred and generally the sounder was even ignorant of the fact that a former course was being crossed or a future sounding section would be taken in the same region. The crossings are therefore representative of the general accuracy of all the soundings.

From the examples given it follows that the mistakes in the soundings are generally much less than 50 m for each of the two sections, especially as the errors in the position also play a part. A comparatively small change of position generally suffices to obtain what is evidently the correct point of crossing. Although a few cases occur on the charts where somewhat larger differences are found they are very few in number.

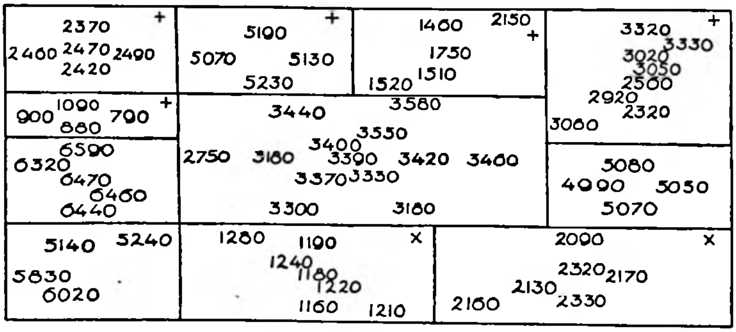


Fig. 2. Illustration of crossings of sounding lines. Those marked with + scale 1: 250.000, with × 1: 500.000, remainder 1: 1000.000.

Not unfrequently contradictions were found between older soundings and our echo-sounding sections. On the whole it has been assumed that our positions were the more accurate.

By comparison of the depths found by wire soundings,

echo soundings and thermometer depth records, taken simultaniously, Maurer (bibl. 80 , p. 64) calculated the accuracy to be $\pm$ 0.6 % to $\pm$ 0.7 %, after some small systematic corrections have been made and a correction for the angle of the wire has been added. This would include the mistakes due to small declivities of the bottom with echo soundings. [No attempt was made to eliminate these by construction (see next paragraph)]. For depths smaller than 5000 m the average inaccuracy would therefore be $\pm$ 25 m, but the larger mistakes sometimes attain 100 m or more. The three methods are supposed to be of the same order of accuracy. Thus Maurer's conclusions are similar to those presented here.

To sum up, we can state the following:

When using the charts and sections of the Snellius Expedition for *geological* purposes we must bear the following in mind:

Wire soundings and echo soundings are of the same order of accuracy. We can safely trust even the deepest soundings to be less than 100 m out. The difference between successive soundings is accurate well within 100 m, that is, only 50 m possible error for each sounding, as systematic mistakes are eliminated when only the difference is considered. Naturally the possibility remains of a greater error having been made in a few exceptional cases. The position of the soundings on the chart is reliable within the area occupied by the central figures of the number on the scale 1: 2.500.000. The distance between the soundings in the sections is sufficiently accurate for mistakes to be ignored in comparison with the possible errors in depth and direction of slope (see next paragraph).

3. CONSTRUCTION OF THE CHART AND SECTIONS.

A. The Chart.

Most of the fair sheets of the new chart were constructed by Lieutenant J. P. H. Perks, while sheets on a larger scale of the Toekangbesi Islands, the Sibutu Passage and the passages round Sawoe and the N. E. Sawoe Sea were constructed by Lieutenant F. Pinke. The combination on a reduced scale and final revision was carried out by Mr. Craandijk of the Department for the Hydrographical Survey under the supervision of Captain J. L. H. Luymes, chief of that Dept. and the leader of the expedition, Mr. P. M. v. Riel.

In drawing conclusions, especially where details are involved, we must be very careful to distinguish between features that are established beyond doubt and the constructions that are still open to doubt. In a following chapter a number of cases will be discussed in which alternative depth curves may be drawn, but these only comprise the more obvious uncertainties. It was our experience that every new line of soundings brought out new features and that they frequently obliged us to alter the track of the depth curves. Again and again ridges or troughs were found to be subdivided or connected with other culminations or depressions further away. It cannot be overemphasized therefore that the present chart is much less complicated than the actual morphology of the sea bottom will eventually prove to be, and that many morphological features of the chart will have to be redrawn.

On the other hand, we have now obtained a very fair idea of the general arrangement of bathymetrical elements. The net of sounding lines is fairly close and little space is left in which major features can still be hidden. A careful survey of the chart shows that the irregularities are not distributed arbitrarily, but that we can distinguish between complicated and simple regions. It is only in the former that fundamental alterations need be expected. In studying the chart it is therefore necessary to know on the basis of which data the curves have been drawn and then to consider whether the feature in question might be essentially altered by new soundings.

Doubt might be felt whether under the given circumstances the chart is not only inaccurate on account of lack of data, but that on the available soundings very different constructions could be made, so that the present chart is too much an expression of personal opinion. Up to a certain extent this is unavoidable. No two people would draw exactly identical curves from the same soundings. One person has a tendency to connect as much as possible the culminations and depressions on neighbouring sounding lines, while the other is more apt to draw isolated banks and basins. Further,

it is possible to follow the direction of the irregularities of a neighbouring coast with the curves as close as the soundings will allow, or the curves can be drawn as straight as possible. Another aspect in which constructions will differ is in the formation of abrupt angular shapes or of soft flowing curves.

Yet another source of variation is the following. For the explanation of a horizontal portion in a sounding section two different shapes of the bottom can be assumed, that is, a ridge crossing the section line obliquely, or a flat terrace. These alternatives are illustrated in fig. 3. A series of soundings A—H is represented together with a wire-sounding K and two possible sets of depth lines. In the first block-diagram a ridge is assumed from the sounding K obliquely across the line of echo soundings. In the lower block-diagram exactly the same sounding line is shown as possible, if a horizontal (or slanting) terrace occurs. K. is here placed on another terrace, but could also indicate a ridge or other elevation.

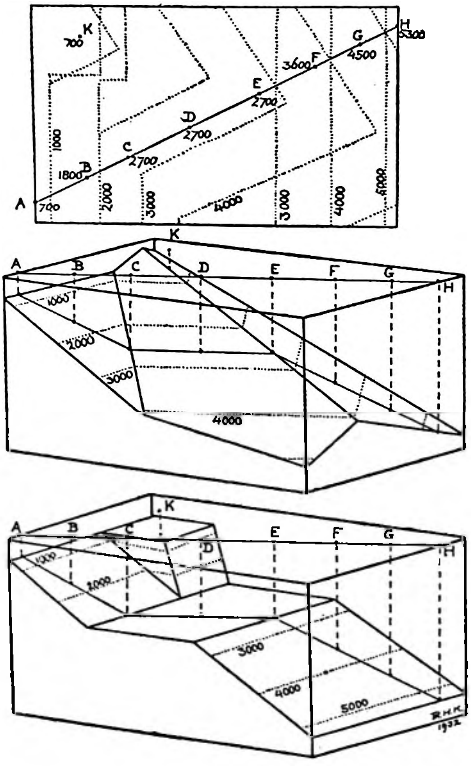


Fig. 3. Chart and block-diagrams to illustrate two different constructions that may be based on the same sounding section.

Finally, two workers may differ in the confidence they show in the accuracy of the soundings. One will mistrust an isolated sounding, which falls outside the general scheme of the depth curves, while the other prefers to assume an irregularity of the sea bottom.

Yet when we come to compare two constructions made by different people, it is surprizing to find how little they differ in the main features. In most cases these differences are moreover the result of inaccuracies or less probable constructions. After a careful comparison in which these are eliminated we found that an almost complete agreement resulted. The remaining differences are almost entirely the result of the style of drawing which cannot influence geological conclusions to any great extent.

We need not fear, therefore, that the chart is a construction with a strong subjective element. Preconceived ideas on the morphology and geological structure of the sea bottom can only play a subordinate part in shapes represented on the chart. In order to obtain the most impartial picture possible from the limited data at our disposal the definite construction was left entirely to the hydrographical workers mentioned above.

It is hardly necessary to point out that although the most logical construction of the depth curves is drawn for the available data, an apparently less logical shape or an as yet unwarranted complication may afterwards be found on increase of knowledge to be correct. As pointed out above we must expect to learn of considerable differences between our present chart and the actual submarine topography, as a result of continued investigation.

It is not only by the number of soundings that the value and reliability of the chart is determined, but also to a large extent by their distribution. An extensive treatment of the reasons for placing the sounding sections in their present positions might be given, but, however important this matter was, during the expedition, it now has only historical value. I will, therefore, restrict myself to a few remarks of general interest.

In the first place the sections were the connecting lines between the successive oceanographical stations and anchoring stations and harbours. Sections at right angles to the depth curves are best

suited to elucidate the morphology. Luckily this is also the most advantageous position for the oceanographical sections. As far as possible the sections were projected, therefore, at right angles to the expected direction of the depth curves. Further, an attempt was made to distribute the sections and stations as evenly as possible over the whole area investigated, but to place them closer in the more complicated regions. In addition to these „necessary" sections, a number of areas of special geological or oceanographical interest were examined separately. The former have already been mentioned. The numerous straits examined and the entrance to the Molukken Sea from the ocean were of special oceanographical interest. When time allowed, extra sounding lines were run between stations and harbours. Double sounding lines were always avoided by altering the course when the same line would otherwise have been followed twice. The great concentration of soundings around Ambon was a result of the numerous visits to that island.

For the construction of the new chart the 200-m line could be drawn in with great accuracy from the existing surveys. The number of existing and new wire soundings outside the 200-m line was nearly 4000. The majority of these are relatively shallow soundings, close to the coasts, executed also for the Hydrographical surveys by the Dutch East Indian Hydrographical Dept., and the Coast and Geodetic Survey of the U.S.A. for the neighbourhood of the Philippines. The number of deeper soundings made by the surveys, former expeditions and cable ships attains only a few hundred. The number of echo soundings used was 33.000. The echo soundings of the German cruisers, the „Emden" „Köln", „Karlsruhe" and „Berlin" and the submarine K. XIII were also used [1]).

An important source of information is further supplied by the temperature and salinity of the water in the various basins. These teach us fairly accurately the depth of the sill over which the water in the depressions is renewed by currents. By this means the depth of a number of straits is fixed, although no — or insufficient — soundings are available. In Chapter II of Part 2, Volume II of the Snellius-Expedition (bibl. 91) van Riel discusses these matters in detail for the entire region of the charts.

In the following chapter the doubtful features in the new chart will be discussed and we shall learn that a great body of work still remains to be done before these can be satisfactorily cleared up. It is sincerely to be hoped that in the future a systematic survey of the deep sea will be added to the programme of the hydrographical survey. In the meantime, however, this defect in our knowledge should not weigh too heavily.

The enormous advance made, especially by the use of echo soundings, is obvious when we compare the old chart with the new one. Countless details and many major features of the morphology of the sea bottom have been brought to light.

I hope to be able to show that this increase in our knowledge of the bathymetry can be made use of for geological purposes and that our science has gained considerably by the extensive new data.

B. The sections.

In geological literature in which references are made to the morphology of the sea floor many improbable ideas are found that were based on a wrong conception of the true angles of submarine slopes. A chart with depth curves is generally apt to give us a very exaggerated idea of the steepness of the declivities. Closely massed depth curves give the impression of precipitous slopes, if insufficient attention is given to the small scale to which they are generally drawn. The greatly compressed sections sometimes published, especially those made for oceanographical purposes, help to establish these erroneous conceptions. Only by examining sections drawn to the same horizontal and vertical scale can we gain a true picture of the forms.

In the report of the „Meteor" Expedition, Volume II (bibl. 80) the sections are reproduced with a vertical exaggeration of 100 ×. Against the advantage of having all the data at hand, must be placed the serious drawback, that the true declivity cannot be even roughly guessed. As the high cost prohibited the inclusion in the present report of all the sections without reduction of the horizontal scale, I preferred making a selection (120 m out of 400 m on the original scale 1: 100.000) of the more important portions and giving these in the correct shape. The true nature of the morphology of the sea floor can thus be deduced directly from the Plates II—VIII at the end of this report.

[1]) The position of the sounding is at the middle of the number on the charts.

It was of great value for the geological interpretation of the soundings that sections to the scale 1: 100.000 were constructed of nearly all the sounding lines made during the expedition. We owe a great debt to Lieutenants Pinke, Veldman and Milo for the care and application with which they executed this very tedious task, for now it was possible to make a choice from all these sections for reproduction on the Plates III—VIII, and for Plate IX the data of all the sections drawn could be made use of.

For the construction of a section from a series of echo soundings the following procedure must be followed. Along the line representing the surface the positions of the ship at the moment the soundings were taken, are marked. The depth noted cannot, however, be set out vertically below these points, because the echo is not returned from a point directly below the ship, but from the point where the perpendicular meets the bottom. Only in the case of a horizontal bottom do the perpendicular and the vertical coincide. Otherwise it lies towards the top of the slope. We must therefore draw an arc with the depth as radius and the ship's position as centre. The tangent to these curves represents as nearly as possible the true section line of the bottom (see fig. 1, p. 7).

An error is made when the ship's course is not in the direction of the slope. The echo is then returned from a point to the side of the section line. Without an apparatus for finding the direction of the echo this error cannot be eliminated. The true shape of the bottom is a tangent surface to the globe drawn with the depth as radius and the position as centre. The true section line falls below the section line found in the construction with arcs, and is also steeper.

CHAPTER III

THE MORPHOLOGY OF THE SEA FLOOR

1. REGIONAL DESCRIPTION OF THE SEA FLOOR [1]).

Lieutenant J. P. H. Perks, besides constructing most of the fair sheets, wrote a memorandum in which a number of alternative constru tions for the depth curves were discussed. Free use was made of this in preparing the figures of Plate I and in writing this section. I wish to express my sincere thanks to Mr. Perks for his kind permission to use his important notes.

It is possible to subdivide the seas in the archipelago into separate basins and areas which form more or less distinct morphological units. Some of the boundaries, however, must necessarily be more or less artificial. We distinguish the following regions:

1) Strait of Makassar, 2) Celebes Sea, 3) Sulu Sea, 4) Molukken Sea, 5) Pacific Ocean, 6) Halmahera Sea, 7) Boeroe-Ceram-Aroe-Timor Trough, 8) Indian Ocean, 9) Outer Banda Arc, 10) Weber Deep and Sawoe Sea, 11) Inner Banda Arc, 12) Banda Sea, 13) Flores Sea.

1) *The Strait of Makassar.* (Pl. IV, section 4—7,11, Pl. I, fig, 1, and fig. 19).

The Strait of Makassar naturally falls into two subdivisions. The southern part which lies between Makassar and the Equator, and the northern part which forms the outlet to the Celebes Sea.

The southern part is characterised by a very flat and horizontal bottom with steep sides. South of the narrow part at 3° S. the bottom lies at about 2000 m; to the north the depth gradually increases to 2500 m. Steps (= morphological flexures) occur on both coasts.

The northern outlet is entirely different from the remainder of the basin. The bottom is not only irregular in detail but is varied by important ridges and basins. The most important feature is a great ridge that projects from the south eastern corner of Cape Mangkalihat half way across the strait. Although it breaks off here there are indications that a ridge of about 2000 m, which is not indicated on the chart, crosses over to the side of Celebes. The number of soundings is not sufficient to determine the shape and course of the irregularities to the north of the Mangkalihat Ridge. On Pl. I, fig. 1 the construction used for the fair sheets of the new chart is reproduced, side by side with another possible shape. Here also, as in all doubtful cases, further soundings may give a very different picture from either of the two alternatives drawn. It would therefore not be wise to found structural theories on the shape of the details.

The deep sounding 2717 m opposite Balikpapan 1°-40′ S, 117°-45′ E is probably a mistake; the actual depth would seem to be about 1700 m.

2) *The Celebes Sea* (Pl. IV, section 8—14 and fig. 18).

The chief characteristic of the Celebes Sea is its flat, unvaried bottom and steep sides. In this respect it bears a strong resemblance to the Strait of Makassar, although the ground plan is entirely different.

[1]) In the following description the depths are not mentioned as a rule for fear of overburdening the text. Reference to the chart will teach those interested in actual figures more than a dry summary of depth records. On Pl. IX the more important steps and steeps are indicated. See also the sections on Pl. II—VIII, and the alternative constructions on Pl. I. For the manner in which oceanographic data have been used, see van Riel (bibl. 91).

The deepest parts of the bottom are found along the southern and north eastern coasts. The southern deep is about 5500 m and runs parallel to the north coast of Celebes at 2°—30′ N. The shape is possibly less regular than that drawn on the chart. One exceptionally deep basin is marked on the American chart with 6220 m maximum depth, to the south west of Mindanao. Other deeps occur along the coast and close to the Kawio Islands. Possibly a deep of about 5000 m lies to the east of the Sibutu Passage.

Minor irregularities were found between 4° and 5° N, and from 123° E to the Kawio Islands, with respectively minimum deep soundings of 4200, 4100 and 2000 m. Especially close to the Kawio Group the bottom is highly irregular. It does not seem unlikely that we are dealing here with submarine volcanoes, especially as one was reported in 1922 at 4° N,—124° E, but possibly there are ridges.

The southern slope up to the coast of Celebes is on the whole gradual. A few tongues running off from the coast in a north-easterly direction are indicated, and at 124° E an elevation that probably belongs to a submarine volcano had already been discovered by former soundings. In detail the sections show minor irregularities especially close to the coast.

The slope from Maratoea and Moearas is steep but regular. On the other hand, between Tarakan and Maratoea the bottom is very irregular. Steep and sloping steps occur and many parts of the sections are irregular. A clear system of ridges and troughs cannot be drawn, although the number of soundings would have been sufficient to show them if they had existed. Block-faulting therefore appears a more likely explanation of the shapes than folding. The simple shape of the 200 m line also proves that the folding of the tertiary strata on the land cannot be directly connected with the elements of submarine topography.

With the exception of the sections running to the Basilan Strait and Sibutu Passage the north-west and north-east slopes are known only from wire-sounding by the U.S. Coast and Geodetic Survey. Apart from the deeps already mentioned the general shape is rather simple.

3) *The Sulu Sea* (Pl. IV, section 15 and fig. 18).

The general formation of the Sulu Sea is well known from the detailed wire-sounding survey of the U. S. Coast and Geodetic Survey. Some information has been added by our echo-sounding section.

A number of parallel troughs and ridges run in a north-easterly direction from Borneo to the Philippines. The Palawan Trough, formerly assumed, does not ex ist. The Palawan Islands, a less well marked trough and the ridge connecting the Cagayanes and Bancoran Banks, the major deep of the Sulu Sea, just over 5000 m deep opposite Basilan Strait, and finally the ridge dividing the Sulu Sea from the Celebes Sea. The latter ridge is more or less subdivided: the north-west part links up with the northern cape of British Borneo; the south-east part which bears the major islands is continued in the southern cape of British Borneo and is mainly composed of tertiary volcanoes as well as reefs. Steps were found at the end of the Basilan Strait and to the west of Pearl Bank.

The Sibutu Passage is known from wire and echo soundings. The morphology is not striking. Submarine erosion caused by the strong tidal currents, superimposed on subaerial erosion forms, probably dominates the shape of the contours. The drop to the Celebes Sea is far more precipitous than that towards the north-west.

4) *The Molukken Sea* (Pl. III, section III; Pl. IV, section 16—21; Pl. V, section 22—23; Pl. I, fig. 2, and fig. 20, 23, 25).

Morphologically the Molukken Sea comprises the region between Celebes and Halmahera, the Soela Islands and Mindanao. The morphology is highly complicated both in its major features and in detail. Nevertheless, the general plan is fairly simple: a number of ridges and troughs run from Mindanao to the south, bending away to the west on the south-west side and in a lesser degree to the east on the south-east side.

Minahassa, the volcanic peninsular of Celebes, is connected to Mindanao by a broad, flat ridge with many recent volcanoes. Apart from the volcanoes the shape is fairly simple. East and north of the Kawio Islands and the eastern slope is somewhat complicated.

The gulf between the two southern arms of Mindanao is the northern continuation of the

Sangihe Trough (= Sangir), a deep basin between the Talaud Ridge and the Sangihe Ridge. To the south this basin can be traced as a narrow deep trough running close beside the Sangihe Ridge and the Minahassa right on to the Gorontalo Basin in the Gulf of Tomini. A second trough touching it like the legs of an X runs from Miangas along the Talaud Islands as a slight depression and into the Morotai Basin. In the north they are divided by a southerly tongue of the submarine ridge from Miangas to Cape San Agustin (It is possible that this ridge crosses over to the Sangihe Ridge in the form of an indistinct swell). The western tongue is remarkably regular in breadth and depth. On the chart it is not connected directly with the Gorontalo Deep, but as a matter of fact it is also possible that the connection is perfectly straightforward (Pl. I, fig. 2). In the Gorontalo Deep the shape changes from a narrow trough to a broad flat basin with a very flat bottom that continues into the Gulf of Tomini also with a flat though less deep bottom.

The Talaud Islands are connected with Miangas and Cape San Agustin by a ridge. The Nenoesa Islands lie on the southern extremity of a more or less independent ridge along the east side of the Talaud Ridge. The separate tongue into the Sangihe Trough has already been mentioned. These ridges are characterized by great irregularity that is caused by a subdivision into more or less distinct parallel ridges. Although the relief of these is small compared with the whole ridge, the sections swing up and down considerably. The southern islands are continued in a ridge that drops down gradually into the Morotai Basin. This ridge lies beside the deep on its south-west side, running to the Morotai Basin mentioned above.

The central part of the Molukken Sea is the most complicated of all. As a whole it forms a broad-backed ridge down the middle of the passage, bending away to the south-west in the direction of the Banggai Archipelago. The northern end is the southern division between the legs of the X-shape already mentioned. Although the possibility of a connection with the Talaud Ridge is not excluded the available data favour the existence of a dividing trough that is only of slight depth, however. Nevertheless, if we take a sufficiently broad view of the major features the Talaud Ridge is the continuation of the ridge under discussion. In the same way the southern end may or may not be directly connected with the Banggai-Soela Ridge, but they are doubtless linked together to a certain extent.

The surface of this major ridge is subdivided into a number of minor ridges and troughs of varying length and distinctness. In this way the two islands Majoe and Tifore appear as the highest points of two small ridges which interchange but are not connected. The number of ridges varies between four and six for different cross-sections, with a corresponding number of troughs between. The comparative shortness of these features makes it impossible to draw the connections with certainty, based on the half dozen sounding sections at our disposal (see for instance fig. 2 on Pl. I). The relief is without doubt more complicated than we are yet able to indicate on the chart.

East of this central region a series of oblong troughs follow the Halmahera geanticline. The deep Batjan Basin in the south is irregular in shape and is subdivided by a north-to-south ridge, the west half by an east-to-west ridge, both of minor importance. The narrow trough to the north runs out into the deep basin of the Morotai Deep, but is continued less distinctly along the south-west side of the Talaud Islands as we have already indicated, and on the east side of the same group to the latitude of Miangas. The probable minimum depth is assumed from oceanographical data. Although a minor complication is found to the west of Ternate this series of deeps and troughs is strikingly similar to the corresponding series on the west side of the Molukken Sea. In fact, the whole region under discussion is surprisingly symmetrical: between the major volcanic geanticlines in the east and west which are convex towards eachother, lies a broad geanticline from Talaud to the south. The intervening troughs are narrow in the middle and broaden out at both ends.

Little is known with regard to the region just north of the Soela Islands. It looks as if a deep runs along the north coast of these islands, with maximum depth of 3500 m at the east end.

In direct continuation to the north of the Halmahera geanticline are a few unimportant ridges connected with the island itself, the North Loloda Islands and Rao. Possibly they are connected with the curious bulge running round the northern point of Morotai. The latter island is the slightly irregular continuation of the Halmahera geanticline. A narrow deep-lying ridge runs off north to the great Snellius Ridge that passes up north along the Talaud Ridge. In the south it is broad and bears two culminations side by side. To the north it tapers out and slowly sinks down, losing its

individuality at the latitude of Miangas. The broad flat between 3000 and 3500 m on the eastern slope possibly runs on uninterrupted to the corresponding feature east of Cape San Agustin.

5) *The Pacific Ocean* (Pl. V, section 25—30; Pl. VIII, section 24; Pl. I, fig. 3, 4, and fig. 23).

Only the southern end of the great Mindanao Trough and the adjoining part of the ocean bottom were investigated. The Mindanao Trough is remarkably regular in shape. It is slightly S-shaped, curving in an uninterrupted sweep along the coast of Mindanao, the Snellius Ridge and Morotai. Between 1° and 2° N it shoots off at right angles to the east and probably follows the north coast of New Guinea. The slopes on both sides are moderately steep and show a fairly constant degree of declivity. Irregularities occur but they are subordinate in importance to the general regularity. They are found principally between 4° and 5° N. Possibly they are in part caused by mistakes in the position of the soundings (for instance a pit at 4°—45′ N and 128°—25′ E with a depth of 7800 m, based on one older wire sounding, has been discarded on the chart). The dent in the depth curves between 6° and 6 ½° N on the east side are based on a doubtful interpretation of the irregularities in the sounding section to the north-west and may very well prove to be only very slight (fig. 3 Pl. I).

The trough line is also a simple curve. The Emden Deep at 10° N is the deepest spot known on earth and reaches just over 10.000 m. From here to 5° N, a distance of 500 km, the greatest depth remains 9000—10.000 m. (At 7° N there is a fair chance of finding a second spot with more than 10.000 m depth). At 4° N the greatest depth is about 7000 m; at 3° N it is 5500 m, diminishing to 5000 m at 2° N. The deep running east is somewhat less than 5000 m. Whether the Asia Islands and Tobi and Helen reefs are connected by a ridge cutting off this deep from that north of New Guinea is uncertain.

The bottom of the Pacific Ocean in this neighbourhood is between 5000 and 6000 m deep. On the south-east side a series of banks of varying sizes and depths, some with small reefs, connect the Palao Islands with a bank opposite Halmahera.

In the west, before reaching the Mindanao Trough, the bottom rises in a strip of irregular and often considerable mounds. Without additional soundings little can be said of their connection and shape, but we can already state that the height above the adjoining sea bottom varies at least between 1000 and 2000 m and that it is not a simple ridge with an undulating crest line, but that sometimes at least two elevations occur on one section line and that these may point obliquely into the trough (See fig. 3, Pl. I). A parallel may be drawn with the apparently irregular series of elevations south of the Java Trough. Of these even less is known, but the available data point to an intermittent series of elevations.

The western slope of the southern end of the trough shows complications. A ridge appears parallel to the trough opposite Morotai. Either it ends at 1½° N or it continues up to one of the ridges on the southern slope of the easterly arm of the trough. In the latter case a small trough with a depth of nearly 4000 m is cut off at the corner. At the most northerly point where this ridge was sounded (slightly more than 2° N) it has a double crest. It is reasonable to assume that the second part forms the submarine continuation of the north-eastern arm of Halmahera (see fig. 4 on Pl. I).

6) *Halmahera Sea* (Pl. V, section 31).

Only a limited number of wire soundings and one echo sounding section are available to ascertain the shape of the complicated area between Halmahera and New Guinea. The southern part between Batjan, Kofiau, Misool and Obi is better known.

In the north a row of atolls and almost-atolls situated on a broad bank connects the central part of Halmahera with Waigeo. From the temperature of the water it follows that the lowest part in this connection is about 1000 m. On the northern slope down to a depth of 2500 m at 1° N the bottom is highly irregular with a number of secondary ridges.

South of the atoll chain is a basin of moderate depth. The shallower parts may be connected with Poeloe Jiew, possibly they are independent (drowned atolls?).

A very distinct ridge forms the division between this basin and the chief depression of the Halmahera Sea. This ridge is the submarine connection between the eastern arm of Halmahera and Gebe. The continuation to Gagi or Waigeo is probably not deeper than 800 m.

The principal basin lies to the south of this steep, narrow and straight ridge. On the whole

the bottom appears to be flat, but one mound was crossed with the echo sounding section. This elevation may be connected with the southern part of Gebe.

The Wiedie Islands and Jef Doif take up the south western and south eastern corners of the basin; its southern border is formed by the string of islands between the end of the southern arm of Halmahera and Kofiau. It is not known to what extent the ridge is continuous, although the temperature of the water fixes the minimum depth between 500 and 1000 m. West of the Boo Islands a slight interchange of two ridges is indicated.

There is not a shallow and direct connection between Obi and its northern neighbours. A rather deep trough lies between it and Batjan and in all probability also Halmahera. In the former case two doubtful, deep ridges are marked on the chart. The most direct connection would appear to run from the eastern corner to Kekeh, Pisang and the Boo Group and/or Kofiau. This ridge is probably more distinct than appears at present on the chart. A fairly deep and straight trough follows along the south-eastern side of this ridge. Whether the eastern end runs on beyond Kofiau into the narrow and shallow passage between Batanta and Salawati still remains to be seen. The west end tails off between Obi and Gomoemoe.

7) *The Boeroe-, Ceram-, Aroe- and Timor Troughs* (Pl. III, section IV—VIII, X; Pl. V, section 35—39; Pl. VI, section 40—46; Pl. I, fig. 5, and fig. 22, 24).

The longest trough of the eastern part of the Archipelago is found on the convex side of the Outer Banda Arc. It is also the trough the most simple in shape, especially the part south of the Aroe Islands. Depth and breadth vary within rather narrow limits. The regular sweeping curve is only interrupted north of the Aroe Islands by a sharp bend. Along most of its length the trough is also free from secondary ridges and deeps. Only between Ceram and Kai (=Kei) a subdivision should be noted. The slopes are seldom steep and a broad flat bottom is found hardly anywhere.

The deepest part is situated to the north of Boeroe and West- Ceram and consists of three separate deeps. The first has a direction from south to north and runs along Sanana and the Sanana Ridge. A slight swell probably divides it from the second and deepest basin, north of Boeroe. This swell broadens to a bank in the north. The basin has a direction from east to west in the centre of the Boeroe Sea. A ridge with probable maximum depth of 3900 m forms the division towards the deep north of West-Ceram. This deep is complicated in shape, with a longer axis from south-west to north-east. The irregularities are caused by adjoining ridges and two small pits. Towards the east the depth gradually decreases from 4000 m to less than 2000 m at the east end of Ceram.

To the north of these deeps we find an indirect connection between the Soela Islands and Misool. Mangole and Lifamatola tail off to the east in a short broad ridge halfway across the Obi Strait. To the south a small trough, possibly with an outlet of 2000 m to the east, lies between another ridge running from the south coast of Lifamatola with an S-shape down into the Boeroe Deep. Possibly this ridge continues eastwards as far as 127°—50′ E and 2°—10′ S.

Eastwards the connection of the Soela Ridge is continued by a ridge rising out of the Molukken Sea (possibly connected with the central ridge of that region). It is divided from the Lifamatola Ridge by a passage of 1900 m probable minimum depth and is linked to the small island near Gomoemoe.

Gomoemoe itself lies on a broad, indistinct ridge running from the south-west and not very definitely joined to the ridge from the south coast of Lifamatola. A very distinct ridge runs off east from Gomoemoe to the south of the deep already mentioned that lies south-east of Obi and Kofiau. The crest line of the narrow, straight and fairly steep ridge swings up and down between 500 and 1300 m. At its eastern end it splits into two and is attached to the northern slope of a new ridge. The latter is the broad and flat westerly continuation of the flat of Misool, so that an indirect connection between this island and Obi and the Soela Islands is effectuated. This ridge begins south of the east end of Obi in the Ceram Deep and rises up steeply. Besides the branch running towards Misool there is a more southerly elevation parallel to the main trough.

The southern slope of the Boeroe and Ceram Deeps is steep to about 2000 m and then more gradual to the coast with irregular, discontinuous and not very pronounced ridges. Opposite the Strait of Manipa complications arise in the shape of a transverse deep and a small deep trough along the north-west of the island Boano.

Between Misool and the Kai Islands the trough is not built on the simple undivided plan shown elsewhere. Four to five troughs are found on some cross sections, on others less. The plan is definitely built on a strike along the axis of the trough, but the individual features appear to be limited in length. Their height or depth or breadth is subject to great variations. As a result the considerable number of cross sections is as yet insufficient to fix all communications and interchanges of the minor features. The portion north of the Kai Group especially is still very far from satisfactorily cleared up. Some idea of the possibilities between which a choice cannot be made, however important it would be from a theoretical point of view, may be gained from fig. 5, Pl. I.

Between the Kai and Aroe Islands lies the deep Aroe Basin with a flat bottom and very steep slopes. Of the northern end nothing is definitely known. On the east it rises up directly to the flat around the Aroe Islands; to the west it is fringed by an important submarine ridge. Whether and how this ridge continues on to New Guinea or the ridges in the Ceram Trough still remains to be ascertained (see fig. 5, Pl. I). It is indistinctly subdivided. A narrow trough that is shallow in the middle and of considerable depth to the north and especially to the south lies between the ridge and Noehoetjoet (= Groot Kai).

The main trough bends round the southern end of the ridge and continues along the Outer Banda Arc. From this point complications have practically vanished. East of the Tanimbar Islands the trough is of consistent depth and breadth, almost perfectly symmetrical and straight. It bends slightly round the south end of the group and then slowly deepens and broadens out into the Timor Trough. West of the Aroe Islands the following maximum depths on the cross sections were sounded: 1690, 1600, 1620, 2420, 3310, 2650, 2610.

Although the 200 m line, and therefore what is generally considered to be the edge of the continent, shows a great indenture opposite the Tanimbar Group the 300 m line already nearly follows the line of the trough in an almost straight line. The 500 m line marks the beginning of the slope into the trough and therefore the true edge of the continent is a line that runs practically parallel to the central trough line.

The steepness of the trough opposite Timor differs little from that near Jamdena; it is only by the greater breadth that the superior depth is attained. Almost all the evidence for the curious complication on the north slope of the central region is an older wire sounding. If either depth or position prove to be wrong the trough would be almost as simple in shape here as further along. Apart from this doubtful detail the Timor Trough is of great regularity, straight, almost symmetrical with even, gradual slopes on both sides. Some very slight swells were found on the bottom between 127° and 128° E, and a few scattered, local irregularities (see Plate IX).

Both depth and breadth of the Timor Trough gradually decrease as one approaches the south-west end, while the slopes retain the same character and declivity. Although minor irregularities were found the general shape remains a simple, undivided trough. The end is reached south of Roti where the depth has decreased to less than 2000 m. In this place a double sill is formed with a small depression between.

8) *The Indian Ocean* (Pl. VI section 47—51, and fig. 23).

At the end of this volume some remarks will be found on the results of the echo soundings in the Red Sea and in the Indian Ocean between Africa and Sumatra. Along the coast of Sumatra and Java a number of echo soundings was procured by submarines of the Royal Dutch Navy, many of which were carried out by the gravity survey expeditions of Vening Meinesz.

From these soundings it follows that the comparatively flat bottom of the ocean rises up in a series of disconnected banks (one bearing Christmas Island, another Corona Reef) along the edge of the great trough that runs parallel to the coast. The trough itself is deepest south of Java where it reaches a depth of over 7000 m. The shape is simple both in section and ground plan. On the inner side a ridge is found that bears a number of islands west of Sumatra, but which nowhere reaches a height of more than 2000 m below sea level south of Java. Between this ridge and the main islands a narrow and not very deep trough is found.

The soundings of the Snellius Expedition are concentrated in the north-east corner of the Indian Ocean. After two sections had proved that the Java Trough continues far towards the east the desirability of ascertaining whether it is connected with the Timor Trough was felt. Prof

Vening Meinesz encouraged this undertaking, pointing out the importance of also investigating the connection between the ridge south of Java and the island Sawoe. Four additional sections were therefore made which bring out the general plan with satisfactory clearness.

The Java Trough continues directly into the Timor Trough with a gentle loop southward. The depth decreases very regularly and gradually from the deep south of Java to the sill already mentioned to the south of Roti. Its breadth throughout is some 40 km. The depth is everywhere about 1000 to 2000 m more than the sea floor to the south. On the whole the elevations on the ocean side are slight.

The ridge south of Java decreases in height to a point south of the west end of Soemba, where the depth is about 4000 m. From there it gradually rises up again and continues to the island Sawoe. On the north side there is a distinct deep of about 4500 m which appears to be more or less subdivided by a ridge running north-west to south-east to the south of Soembawa's western end. The depression runs along the north side of the Sawoe Ridge with diminishing depth into the Strait of Sawoe.

The broad, flat swell of the island Soemba loses itself in the region north of this depression, tailing off in a broad, indistinct bank, reaching no further than 117½° E. An inconspicuous depression lies between this bank and the Lesser Soenda Isles which shows sufficiently that there is not a direct morphological continuation of Soemba to Java, Bali, Lombok or Soembawa.

9) *The Outer Banda Arc* (Pl. III, Section XX; Pl. V, section 32—34; Pl. VI, section 52—59; Pl. I, fig. 6).

Morphologically the Outer Banda Arc begins in Soela Sanana, north of Boeroe. It continues as far as Sawoe and then splits into two arms, one running to Soemba, the other into the ridge south of Java.

The shape of the Outer Banda Arc is very much more complicated than that of the troughs along the convex and concave sides. The slopes of Sanana are very steep as far as they are known. The island continues to the south in a broad ridge with a very complicated structure both in its general shape and in the details of the sections. The principal mass bends away to the south-west and in all probability continues in the narrow ridge to the west of Boeroe. A rather insignificant ridge forms the direct connection with this island. In its northern part the bank is subdivided into three culminations that slope down slowly southwards. Soon a fourth ridge is added on the west side that links the Sanana Ridge to the ridge west of Boeroe. The link with Boeroe seems to branch off from the west side of the Sanana Ridge. Its deepest point is about 3000 m and from there it rises up quickly to the north-west point of Boeroe, sending off a branch to the east along the north coast of this island.

With the exception of the north-east part Boeroe dives down steeply into considerable depths on all sides. A bulge at the south-west side is connected on the chart to a ridge running southwards, but further soundings are required to ascertain this definitely.

Ambalaoe forms the highest point of a ridge that springs from the south-east corner of Boeroe. From the data at hand the connection is, however, as ill defined as the narrowness of the strait between the islands could allow.

From Ambalaoe the ridge curves to the east up to Ambon, but the connection is not quite straightforward. Two banks are formed on the ridge, one of about 1600 m, the other slightly less than 1000 m. Both of these are situated on more or less individual portions of the ridge. The degree in which these parts interchange is not yet sufficiently fixed. The last bank forms the end of the ridge. It is not directly connected with Ambon due north but touches the foundation of this island at a depth of 1700 m.

The southern peninsular of Ambon has a more pronounced continuation to the west in a bank running down into the basin of the Strait of Manipa. The continuation of Ambon to the east is clearly found in the Oeliassers, that are situated on a bank which is less than 200 m deep. The channel to the north is only from 300 to 800 m deep. The bank itself pitches down to the east, disappearing at about 129°—25′ E. An arm runs off from Saparoea and (or) Noesa Laoet to the south east which appears to connect the Oeliassers and Banda, while it also touches the end of the Siboga Ridges. The deep depression of about 4000 m in the Strait of Manipa is roughly three-cornered in shape. The apex is close to the island from which its name is derived, while the base is formed by the ridge from Ambalaoe to Ambon. Exactly in the centre is a steep, almost conical-shaped bank, which rises up to less than 1000 m in depth. Six sounding lines running to the apex

show the form accurately. From the simple shape and steep slopes we may conclude that it is almost certainly a volcanic cone. It is the highest point on a very indistinct ridge cutting through the basin and forming a second connection between Ambon and Ambalaoe.

There are small, irregular elevations and depressions in the bottom of the basin, but the general plane is flat and horizontal. The sides rise steeply up, except in the south to the Ambalaoe Ridge.

In the north the Manipa Basin ends abruptly, but a fairly shallow tongue runs off along the north coast of Manipa into the deep trough beside Boano with a series of shallow banks along its north-west margin.

Little need be said concerning the arc itself between Ceram and the Tanimbar Group. Everywhere it is shallow with a steep or very steep slope towards the concave side, where also most of the islands are concentrated. The more complicated north-east slope has already been discussed.

The part between the Tanimbar Group and Timor is highly complicated. The number of soundings is considerable and is sufficient to bring out the main features of the structure. Many details still require renewed investigations (see fig. 43). Although the connection between the two troughs at both sides is never deeper than 1600 m and the ridge as a whole is therefore continuous, a more detailed analysis of the forms shows that the interchanging of the crests of a number of secondary elevations is the ruling feature of the structure. The northern slope is steepest and comparatively straight, with a string of banks to the north of Babar. One of these emerges in the island Dai. In how far these are connected is still uncertain. Another open question is the connection with Babar or Daweloor as well as the connection with the row of islands west of Jamdena. At any rate there seems to be a branch diving down into the Weber Deep behind this group (see Pl. I, fig. 6).

Babar is also linked up with Jamdena by a southerly bank, but its exact shape is still uncertain. On the other hand it is clear that both Masela and Babar are situated on minor ridges pitching to the west. The latter ridge is straight and narrow and clearly marked along 8° S as far as the island Kalapa. The ridge of Masela is broader and curves gradually to the west in a more southerly direction, losing its individuality at about 128½° E. It soon emerges again in an elevation crowned by the atoll MeatyMiarang, east of Lakor. A more northerly branch passes to Sermata and its neighbours.

The relation between the latter island and Meaty Miarang is not yet known.

This atoll belongs to a ridge running as far as Leti. A not very distinct and quite insignificant branch runs off from the north-west end in the direction of Kisar; the rest ends at Leti. There is a definite narrow trough which clearly marks an interchange between the Leti Ridge and Timor. This island begins somewhere south of Moa in a broad bank rising up slowly towards the east end of the island.

Over the whole length of Timor the rule holds that the northern slope of the arc is much steeper than the southern. Roti, however, is steepest to the south and for the remainder of the arc the slopes are all more gradual.

Dao Strait and Sawoe Strait are very much alike. Both are only about 1000 m deep in the shallowest part of the centre and both show a trench in the centre running from southwest to north-east. In Dao Strait this trench is indistinctly divided into two parts of about equal size. The northern part ends on the slope into the Sawoe Sea, the southern part runs down into the end of the Java Trough (possibly not quite directly).

In Sawoe Strait, on the other hand, the division lies close to the edge of the Sawoe Sea, so that the north-east part of the trench is exceedingly small. The southwest portion, on the other hand, is very distinct and follows down directly into the trough, north of the ridge running on to the bank south of Java.

Minor swells in the straits are found which seem to follow the same oblique position with regard to the arc as the trenches. This is most fully developed in Sawoe Strait.

It is interesting to note the absence of important scarps in these straits.

The breadth of the Outer Banda Arc varies considerably. From Sanana to Boeroe it is narrow and again between Ceram and the Kai Islands it narrows down. Here the breadth suddenly increases about four times. Then it is again of medium breadth up to Jamdena. It is at its narrowest after the complicated area round Babar. Along Timor it widens again, narrowing down at Roti.

10) *Weber Deep and Sawoe Sea,* (Pl. III, section IX, XI; Pl. VII, section 60—73, 81, 85—89, and fig. 21, 42).

From a morphological point of view the Weber Deep and Sawoe Sea form one single element, namely the deep between the Inner and Outer Banda Arc. This region is on the whole exceedingly simple in formation. Although the depth varies between wide limits, the cross sections are everywhere remarkably uniform and simple. Both slopes are very steep, straight and not complicated by secondary elevations. The bottom is flat and horizontal. The only serious complications are concentrated in the region round Kisar.

The bank on which Ambon and the Oeliassers lie is more or less connected with Banda. In this corner the Weber Deep begins directly with considerable depths which increase on rounding the corner at Banda. The section is here still that of a normal trough without a flat central portion, with small irregularities.

South of 5° S. the flat central part sets in at a depth of 7000 m. East of Manoek depths of 7500 may be expected (maximum depth sounded 7440 m). The part which is more than 7000 m deep takes up about half the entire breadth here. The depth then gradually decreases to 4500 m north of Meaty Miarang.

During the following 200 km there is an abrupt change in the character of the trough by the sudden appearance of important ridges in the centre, culminating in the island Kisar. The main feature is a bank, broad in the east and narrowing towards the west. Two branches of the trough run along the north and south sides, but they fail to bring about a connection with the remainder of the trough deeper than about 2500 m. This is caused by the welding together of the central ridge both with Kisar on the south and Romang on the north on the Inner Arc, while the former island is connected with the Outer Arc by an insignificant ridge off the Leti-Moa bank.

To the west of Kisar the ridges disappear as suddenly as they sprang up. Again the trough is undivided with very steep slopes and a remarkably flat bottom. This remains during the entire stretch from Kisar to Soemba, with the exception of the slight swell to the south of Kambing where the two arcs very nearly touch.

Few basins are so simple in shape as the Sawoe Sea. The bottom is perfectly flat and horizontal. The south and southwest slopes are gradual, in the north and east they are precipitous. South of Pantar and west of Dilly on Timor short banks protrude into the basin.

11) *The Inner Banda Arc.* (Pl. VII, section 86—89; Pl. VIII, section 90—91).

The course of the Inner Banda Arc is clear from Java as far as the island Banda, but some doubt exists how it continues from there towards the northwest. There is certainly no direct connection with the Siboga Ridges, but an arm runs off from the northwest that in all probability forms a connection with Noesa Laoet or with Saparoea of the Oeliasser Group. This arm comes into contact with the north end of the Siboga Ridges, but does not appear to join on here.

From Banda to Wetar the string of Banda Sea volcanoes clearly shows the direction of the arc and from Wetar to Java the same row runs on uninterrupted in an almost straight line. If the volcanic piles could be lifted from the ridge, only an inconspicuous elevation would remain, but from the sounding sections we are justified in supposing that there exists at least a slight bank of a rather complicated morphology and composed of non-volcanic matter on which the volcanoes are built up.

The bank on which are situated the Banda Islands stretches far to the north, so that the Weber Deep is narrow here. Its surface is irregular in a marked degree. Besides the arm already mentioned towards the northwest another ridge runs off from the east side in a southeasterly direction. Possibly it is continued as far as the latitude of Manoek, where it is lost in the bottom of the Weber Deep. Otherwise the Banda plateau falls away steeply on all sides. It is improbable that the ridges branching off from the arc on the concave side near Manoek form a direct connection with Banda.

Half way between Manoek and Banda an elevation is found. As the northwest slope is steep it is probable that we are dealing with a submarine volcano of the arc. The minimum depth sounded to the north is 4100 m, so that the non-volcanic part is very inconspicuous here. To the south east of Manoek a shallow sounding probably indicates the position of another, submerged volcano, while part of the elevations further to the west may possibly have a similar cause.

Between Seroea and Damar there are no deep soundings, so that not much importance can be

attached to the drawing of the chart here. It is not improbable that irregularities will be found on the inner side of the arc analogous to those behind Seroea and Damar.

The sections between Damar and Romang show that the non-volcanic ridge is subdivided in the east and is at least 3500 m deep in the middle of the strait. The next strait has not been sounded.

From Wetar to Java and Sumatra the volcanic arc continues in a series of islands with many active and extinct volcanoes. The straits are all less than 1000 m deep and only a few exceed 200 m. The conclusion to be drawn from the geology of the islands is that the morphological elevation is not the result of recent volcanic activity alone, although it has added considerably to its bulk. There is, however, also an elevation of the substratum, for marine tertiary and older rocks are of widespread occurrence.

The submarine slopes of the arc are on the whole very steep along the north side, as far as East-Soembawa, and along the south side, as far as the middle of Flores. They are also remarkably straight, although further soundings may bring out important irregularities. East of Sangeang a steep elevation was sounded, that no doubt belongs to a submarine volcanic cone. Northwest of the same island there is possibly another submerged volcano.

12) *The Banda Sea* (Pl. III, section XII—XIV; Pl. VII, section 74—84; Pl. VIII, section 90—93; Pl. I, fig. 7, and fig. 18).

Under this heading we will discuss the region between the Soela Islands, Boeroe, the Inner Banda Arc, Komba, the Toekangbesi Islands and the southwest peninsula of Celebes.

This region is not one simple morphological unit, but naturally falls into three distinct sub-regions: A. The northwest basin, B. The region between the Toekangbesi Islands, Boeroe and Banda and C. The southern basin.

A. *The northwest basin.*

This basin is deep with an undulating bottom and steep sides on the east, north and southwest; the northwest corner is unexplored. The structure of the bottom must be fairly complicated, for although several sounding sections were run, a clear plan did not emerge. The connections drawn on the chart must therefore not be taken as ascertained beyond any doubt. On the contrary a considerable addition to the sounding data is needed before anything like a definite arrangement of elevations and depressions could be carried out. However, this is not a serious matter, for the size of the elevations is not large compared with the dimensions and depth of the basin. Moreover, if a straightforward plan of arrangement had existed which could have given a clue to the way in which the relief was formed this would already have shown in the present sounding sections. We are pretty safe in concluding that the morphological structure is irregular and the relief unimportant.

The principal features so far ascertained are the ridge already mentioned from the Sanana Ridge round the northwest corner of Boeroe, deep depressions along the northwest and south margin of the basin, a ridge of unknown dimensions in the centre which either runs from Wowoni to the east about half way across to Boeroe or is a loose elevation, a ridge parallel to the edge of the basin to the northeast of Wowoni, a broad bank in the northeast corner of the basin, a series of ridges and troughs in the southeast parallel to the coast of Boeroe.

B. *The region between the Toekangbesi Islands, Boeroe and Banda.*

This part of the Banda basin is characterized by a very complicated and varied relief. Up to the present the existence of only one great bank was suspected, the Siboga Ridge, on which the Schildpad- and Lucipara Islands were situated. The echo soundings have brought to light that the Siboga Ridge consists of several independent ridges and that several other, hitherto unknown elevations and ridges throw the bottom into strong relief. Many sounding sections were run here in an attempt to elucidate the principal features of the morphology, but the complications are so numerous and the structure so diversified that various constructions of the depth curves are still possible (Pl. I, fig. 7).

The following features have been ascertained. The Lucipara- and Schildpad Islands rise abruptly from the centre of the same ridge. This ridge is not directly connected with the bank to the northeast, for a trough with more than 4000 m of water lies between. The second part of the former Siboga Ridge runs parallel to the first, but lies further to the northeast. It is subdivided into

several ridges that appear to converge into one broad bank opposite the Banda Group, from which point they remain separated by a deep of more than 4000 m. To the north-east they are close up against the ridge coming from the Oeliassers and running towards the east. Towards the north-west they are separated from the Ambalaoe-Ambon Ridge by a trough, nearly 5000 m deep.

The Luymes Ridges are one of the most important discoveries of the expedition (called after Captain J. L. H. Luymes, chief of the Hydrographical Department of the Royal Dutch Navy and spiritual father of the Snellius Expedition). They are situated due south of Boeroe and separated from this island by a deep of more than 5000 m. Possibly the ridge already mentioned which rises from the south-west corner of Boeroe connects Boeroe with the Luymes Ridges. Two culminations of the Luymes Ridges are found in the west, while several secondary ridges appear at the east end. The alternative maps on fig. 7, Pl. I, show how uncertain the relation still is between the various elevations. It will also be seen that the banklet due north of the Lucipara Islands may form either the continuation of the northern Siboga Ridge or a more or less separate elevation.

The data on which the construction between the southern Siboga Ridge (Lucipara Ridge) and the Toekangbesi Group had to be carried out, are scarce. This Lucipara Ridge was subdivided by a second ridge running along the southwest margin. The simplest construction was to carry on the structure with a general direction from northeast to southeast, thus gradually accomodating it to the direction of the Toekangbesi Group.

Along the northeast edge of the bank which bears the Toekangbesi Islands runs a deep trough, more than 5000 m deep, followed by a bank less than 2500 m deep. An undulating and fairly deep region follows to the northeast and east.

C. *The southern basin.*

The southern basin of the Banda Sea follows the concave side of the Inner Banda Arc. It is deep, with an average of 5000 m, with few complications, and thus strongly recalls the northwest basin of the Banda Sea. In the centre the isolated volcanic peak of the Goenoeng Api near Wetar rises abruptly from the bottom and is probably not situated on a ridge. Otherwise the relief of the basin is restricted to elevations and depressions between 4000 and 5000 m. The absence of a single correlation even with neighbouring sections shows how intricate this relatively unimportant relief must be. The interchanging ridges to the north and the complications on the Inner Banda Arc cause the ground plan of the basin to be intricate. Its size is greatly reduced as compared with the extension given to it on former charts.

No definite limit can be given to the southwest end of the basin. A group of small banks to the south of the Toekangbesi Group could be taken as marking off the end of the basin.

13) *The Flores Sea* (Pl. III, section XV—XIX; Pl. VIII, section 94—109 [1]); Pl. I, fig. 8 and 9, and fig. 20).

The Flores Sea is situated between the volcanic Lesser Soenda Islands in the south and Celebes in the north. Here again a natural subdivision will help us in our description: A) the shallow region between Java and Celebes, B) the central Flores Basin, C) the ridges connecting Celebes with East-Flores, D) the Gulf of Bone and its continuation towards the southeast.

A) *The shallow region between Java and Celebes.*

A very broad bank forms a link between the southern peninsula of Celebes and the Soenda Shelf to the northeast of Java and Madoera. On this bank many atolls are found. A distinct trough-like depression follows along the edge of the Soenda Shelf, causing a connection between the deeper parts of the Flores Sea and the Strait of Makassar. A second connection runs along the southwest end of Celebes.

When discussing the relation of the atolls to their substratum in Part 2 of the geological results of the Snellius Expedition, it was pointed out that there is a general tendency for these structures to be situated on ridges. One of the best examples is the Paternoster atoll which continues towards the east in a ridge that almost emerges again 50 km further and then slowly pitches down into the Flores Basin in two tongues. In the case of the other groups the position is either less clear or sufficient data are not yet available to form a definite opinion.

[1]) On section 106 a swell is apparently seen, close to the coast of Flores. This is caused by a reverse of the direction of the section over a short distance, as follows from the indications of positions.

B) *The central Flores Basin.*

The central basin is formed by a long trough. Beginning directly at the east end of Java this trough follows along the north coast of Bali, Lombok and Soembawa with ever-increasing depths. The deepest part is over 5000 m and is situated to the north of West-Flores. The end comes rather abruptly towards the east on the 122nd meridian. The northern edge is formed by the shallow banks discussed under separate headings. In the triangle between these banks and the actual trough in the south, the sea bottom is fairly deep and irregular.

The trough is divided into a west shallow basin with very gradual slopes and the principal deep in the east by the narrow passage between the atoll Maria Reigersbergen and Soembawa. The principal trough has been sounded in great detail, some dozen sections crossing it at even intervals. It is nearly straight curving to the north at both ends. The part deeper than 5000 m is of consistent breadth and depth. At the east end a ridge divides the trough into two arms. In the triangular area to the north of the trough we can distinguish a ridge and trough in the same east to west direction and two depressions running north, divided by a bank.

C) *The ridges connecting Celebes with East-Flores.*

Two ridges grow from the south arm of Celebes. The first connects Salajar, Tamboelongang, Tana Djampea, Kalao and Bonerate; the second starts in the flat with a barrier reef on the east coast and connects Tijger atoll with Kalaotoa, beyond which it splits up into two ridges. One of these runs to Angelika flat, the other passes to the northeast of this reef. We have already seen that a ridge divides the east end of the Flores Trough. This ridge passes on to Angelika flat and thence onwards, joining the ridge just mentioned, to the volcano Poeloe Komba, ending in a long, broad and deep-lying bank to the north-east as far as 125° E. Possibly it runs on as far as 126° E. and may even find its way on to Goenoeng Api north of Wetar.

In addition to the ridges already mentioned, Angelika flat is also connected with Flores by two ridges, one running to Soekoen and Paloë, the other to the fish-hook at the end of Flores.

Between the two major ridges a secondary elevation is formed north of Tana Djampea and its submarine continuation to the north. There are insufficient soundings between the remainder of the two ridges to allow definite conclusions to be drawn, but it would appear that the intervening trough is fairly conspicuous. On the whole the slopes of these ridges are steep; the east slope of Salajar is one of the most precipitous in the whole archipelago.

D) *The Gulf of Bone and its continuation towards the southeast.*

The relief of this region is, generally speaking, small. The gulf itself has a flat bottom with moderately steep slopes. To the south lies an area with moderate relief, the main direction of which appears to be from north to south. The principal elevation is the broad bank running south from Boetoeng (= Boeton) on which Batoeata is situated and further south beyond a depression Kakabia. A trough of considerable depth divides this bank from the Tijger Atoll Ridge. Another deep trough the Boetoeng Trough, forms the northeast margin separating it from the bank which bears the Toekangbesi Group. A narrow and steep ridge, the southeast end of which is hypothetical, forms a branch off the Batoeata Bank.

The Toekangbesi Group consists of atolls and islands and reefs without lagoons. These are situated on a broad bank. This bank is connected with Boetoeng by a fairly shallow connection. There are three ridges; the southwest ridge is crowned by atolls, the middle ridge by islands, and the northeast ridge by one island and a reef. The latter ridge continues at both ends beyond the limits of the actual bank. The remaining reefs are placed more or less arbitrarily in the intervening space. The group of banks to the southeast of the group may be regarded as the deeply submerged continuation of the bank. A more detailed description of these islands may be found in the report on the coral reefs in Part 2 of Volume V of the Snellius Reports.

2. FAULT SCARPS ON THE SEA FLOOR.

One of the most interesting bathymetrical discoveries made by the Snellius Expedition was the existence of precipitous slopes in otherwise nearly flat regions, which are interpreted as submarine

fault scarps.[1]). The theoretical importance of these features of the submarine topography is so great that we will discuss at some length the manner in which they are detected by echo soundings.

There are two ways in which a scarp can be found: directly by continuous sounding, or indirectly by construction from a series of soundings. As the former is the most obvious it will be dealt with first.

If we employ the sounding machine continuously while passing over a clear-cut, vertical scarp we should find a sudden increase in depth equal to the depth of the fault. If the fault plane hades, or if irregularities occur at the top or the bottom of the slope no clear echo will be received while crossing the non-horizontal part. The objection has been raised that the echo from the top of the scarp will prevent us from noticing the sudden increase in depth. Thus in fig. 4 we would find the depth at a to be: a-A, at b: b-A instead of b-B, at c: c-A, etc. As, however, the sound wave from b to A is reflected in the direction A-b′ it does not return to the ship; only the wave b-B is received and the correct depth recorded. In the same way, the sound wave from a to D is reflected towards a′, otherwise a double echo would be received at a. If sound-waves did not conform to this rule of reflection we could never receive a clear echo over a flat bottom, because all directions would return an echo, and instead of a short echo a long-drawn rumble, gradually dying out, would be heard. The wonderful sharpness of the echo is illustrated by the fact that when drifting over a horizontal bottom of 5000 m in depth in the Indian Ocean we heard as many as four consecutive echoes from one signal, by reflection against the bottom and the surface of the water.

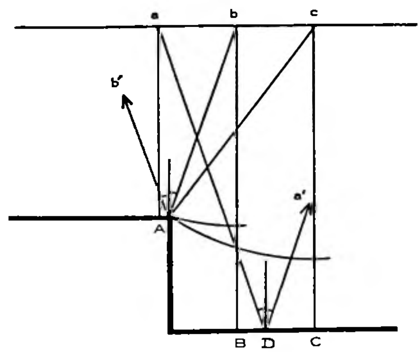


Fig. 4. A submarine scarp with echo soundings.

Fig. 5 shows why no echo is received between a and d, where the bottom is not flat. In e two echoes might be heard.

By this method of continual sounding the scarps were first discovered in the Red Sea to the north of Jebel Teīr. Some sounding sections were being run here in search of a reported bank. By frequent soundings the bottom was found to be comparatively level. During an hour I sounded continually and found the bottom undulated slightly some 100 m. In two cases the echo suddenly disappeared and returned again after a few seconds, 2 to 300 m deeper, to increase slightly and then continue at the new depth for some time unaltered. It had already been inferred from geological data that the Red Sea was probably a faulted region (see i.g. bibl. 89) and this supports the interpretation of these scarps as fault scarps.

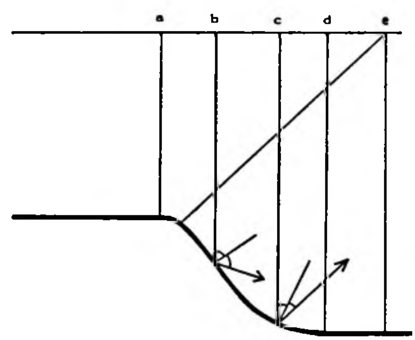


Fig. 5. A submarine morphological flexure with echo soundings.

As it is not possible for practical reasons to sound continually during the whole expedition, and as the probable location of scarps beforehand is not possible either, we are generally reduced to using a series of soundings from which the scarps must be reconstructed afterwards in a drawing. In this case we are at a disadvantage, for if the down-throw is small, the considerable distance between the successive soundings renders it doubtful whether an increase in depth in our section is a scarp or only a moderate slope. Only in cases where the down-throw is several hundred meters the scarp will show clearly in the section. Fig. 6 illustrates these difficulties.

There is also the possibility that the block which has been thrown up is rounded off and that A in fig. 4 does after all return an echo to b and c. If moreover B to C slopes slightly, so that the

[1]) See Tijdschr. K. Nederl. Aardr. Genootschap 1929, p. 534; 1930, p. 809. Scarps, discovered in the West Indies are also attributed to faults by Hess (bibl. 52, p. 34) and Taber (bibl. 112).

vertical sound-wave is not returned, a gradual increase in depth would be sounded even when the successive soundings are relatively close together. A considerable amount of sedimentation or crumbling of the scarp after the faulting will therefore tend to obliterate for the sounding an otherwise still considerable scarp.

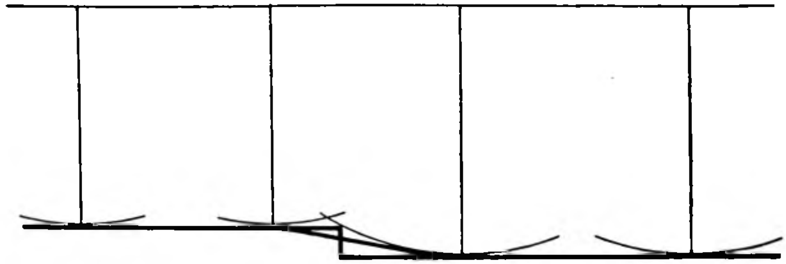


Fig. 6. Two possible constructions, a scarp or a slope, on a series of echo soundings.

Consideration of the above remarks will show that where a change of depth in an echo sounding section is found, especially in an otherwise fairly flat region, it will generally be represented on the section as a slope (i.e. morphological flexure), but that we must always bear in mind the possibility that it is actually a scarp.

When of three successive soundings the second does not reach as far as the intersection point of the arcs belonging to the first and third, we might assume that a mistake has been made and correct the soundings so that they pass through one point (a, fig. 7). Maurer suggested this point of view (bibl. 79). This appears improbable, as the corrections would sometimes be considerable and the echo would have been returned by a very small surface. To my mind most of these cases could be explained far more reasonably by assuming the existence of one or two scarps, as indicated in our fig. 7.

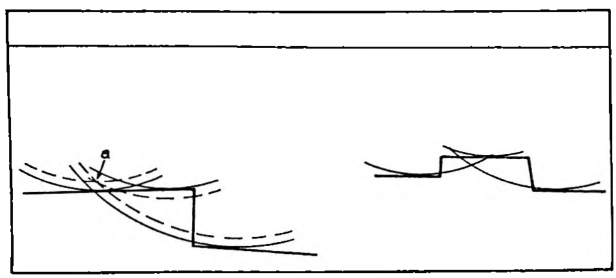


Fig. 7. Apparent mistakes in echo soundings explained by scarps.

In fig. 8 an illustration is given of a series of soundings that can only be interpreted as caused by a scarp or flexure. The soundings a, b, c and d, e, f prove the horizontal nature of the bottom. The drop between c and d may be vertical, but no data are available to show that the slope is greater than that given on the figure.

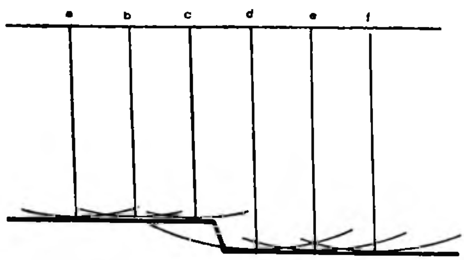


Fig. 8. A series of soundings on the evidence of which a submarine scarp must be constructed.

The objection might be raised against such cases that a mistake has been made in the soundings and that the change in depth is more gradual. As, however, the first-mentioned method of detecting scarps proves their existence without doubt and as there are examples available in which the sounding error would have to be several hundred meters, this objection must be considered too conservative. On the other hand, we must admit that some of the smaller scarps may be the result of errors in the sounding. The opposite case in which a scarp is smoothed off by a too gradual change of depth in the recorded soundings is, as we have just seen, probably a far more common occurrence.

A number of striking examples, constructed from the sounding log of the expedition, are brought together in fig. 9 and 10. Fig. 9 concerns scarps in the Red Sea (from the same series as those described above), as they can be constructed from the recorded soundings. The dotted lines represent the least abrupt construction possible, the full line the more probable shape. The data do

not allow a still bolder interpretation, but it is obvious that vertical drops are perhaps even more probable than the forms represented. An example of this is given by the double line in the middle section. Fig 10 concerns a number of scarps which were detected in the East Indies. In all these

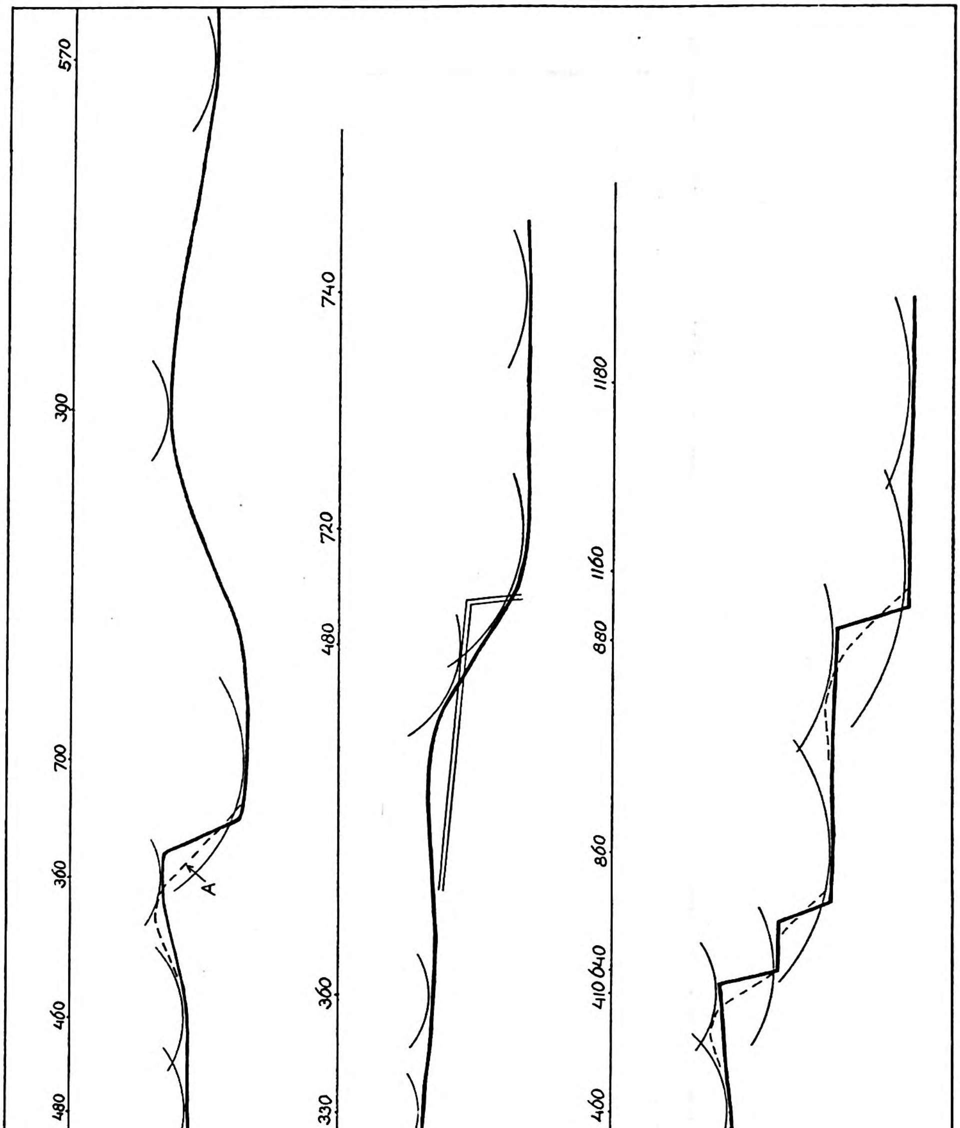


Fig. 9. A number of submarine scarps detected by echo soundings to the north of Jebel Teir in the Red Sea. Scale 1:25.000.

examples, with the possible exception of D, the downthrow is too large to be explained away by errors in the depth records. In E it can be shown that the echo to B was returned from C, for if the dotted continuation of C to the left existed, an echo would have been returned to A, along the line b. As, however, the depth a was recorded at A, the shape of the bottom must be represented approximately by the full line. It will be seen that in all cases a somewhat steeper to vertical scarp may exist and that the slopes may also be slightly less steep, without, however, in any way losing their abrupt character.

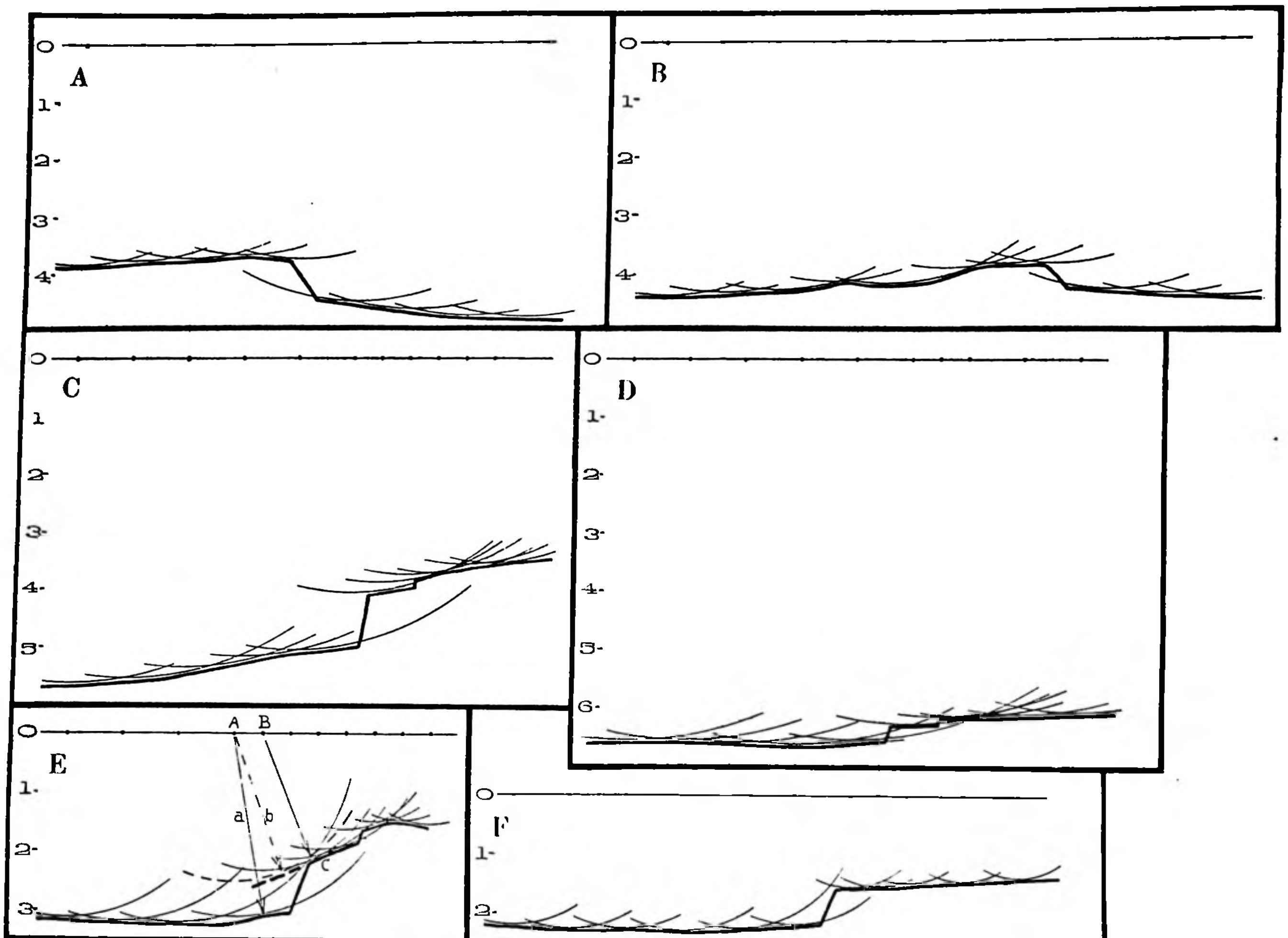


Fig. 10. A number of submarine scarps detected by echo soundings in the East Indies. Scale 1: 100.000. A between St. 331 and Boeroe, B to the south of St. 349, C to the east of St. 366, D to the east of St. 362, E to the southeast of St. 373, F to the east of Babar.

It would, of course, be interesting to repeat some of these soundings by continual sounding, in order to ascertain as accurately as possible the exact amount and steepness of the slopes and also to investigate their horizontal extension and direction.

A number of scarps, in addition to those reproduced here, occur on the echo sounding sections. They are marked on Plate IX. Besides, a great number of morphological flexures were found which are also marked on that same map. It has already been pointed out that many of these may be scarps or stepped faults.

A discussion of the regional distribution of the scarps and steeps will be given later (on p. 47). Here we need only point out that this distribution supports the conclusion that they are not sounding errors. In the first place we note that they are not concentrated in the most irregular parts of the sounding sections. (See for instance the scarps in the otherwise regular sections IX and XII, Plate III, and the irregular southern end of section 30, Plate V, without scarps). This would be the case if they were merely extremely steep parts of otherwise irregular parts of the bottom, or if they were due to errors made by the sounder when the depth varied strongly. In the second place, we will learn later on that scarps and steeps are concentrated in parts of the sea floor where faulting is to be expected from other independent data.

The theoretical importance of these scarps appears to the present author to be considerable. As erosion is practically non-existent on the sea bottom, and as sedimentation is comparatively slow, it is in itself not surprising to find that a fault scarp, once it is produced, remains standing as a bold cliff, in some cases as much as 1000 m high, a feature far more striking than the most pronounced subaerial fault scarps known.

The chief interest of these scarps is that they prove the rigidity of the bottom where they occur and that they demonstrate the ability of a fault to reach gigantic proportions without the aid of erosional obliteration of the load of the thrown-up limb. The former feature especially claims our interest.

In the first place it is of great importance to note that the scarps are not restricted to what might still be claimed as belonging to the sial blocks. In section I and II, Pl. III, examples are given from the middle of the Indian Ocean, from a position where we may safely claim to be in the heart of the sima exposure. The plastic nature of the sima, as claimed by Wegener and others, is not borne out by this characteristic. A considerable thickness must be attributed to the rigid crust of the sima, if it is to sustain a scarp of the given dimensions ($\pm$ 400 m). Perhaps it would be safest in connection with the far-reaching importance of this conclusion to await confirmation of this observation by further echo soundings over the ocean bottom.

The simatic nature of the land-locked basins in the East Indies is doubtful. The existence of scarps in these seems beyond doubt, on account of the considerable number found in the sections.

The second reason for attaching importance to the submarine scarps is the great depth at which they are sometimes found. The hydrostatic pressure at a depth of 5000 to 6000m corresponds to a depth in the solid crust of about 2000 m. It is obvious that the rocks of the sea bottom are not plastic where they form scarps, as these would otherwise flow out to more gradual forms. Of the nature of the rocks on the sea bottom we know next to nothing by direct observation. The deep-lying scarps now prove that the rocks are comparatively rigid, as they are not rendered plastic under an earth-load of 2000 m, and an over-pressure of several tens of kilograms per square centimeter at the base of the scarp (100 m scarp corresponds to about 15 kilograms per square centimeter).

We see that the sima is found to be rigid at its surface and down to a considerable depth in the rock. If these observations are borne out by further investigations an argument against Wegener's theory could here be found.

The importance of the scarps will also be seen when we discuss their distribution, and in connection with the submarine sliding of sediment.

CHAPTER IV

GEOLOGICAL DISCUSSION OF THE SUBMARINE MORPHOLOGY

1. THE GEOLOGICAL STRUCTURE.

A. Introductory remarks.

We are now in a position to essay a structural explanation of the intricate morphological relief of the eastern part of the East Indies. It is not the place in this chapter to advance the possible causes of the formation of the forms. The difference between and the explanation of „Groszfalten", „plis de fonds", geanticlines and oceanic basins are much debated questions. The mere study of a bathymetrical map based on a limited knowledge of the external form of the sea bottom can hardly furnish a solution of these most complicated problems. We will attempt to classify the forms and to determine whether any evidence is given as to the probable structural character and the possible manner of production. We will also consider the question whether modern geosynclines are found among the various basins. It will be attempted to make the following treatment of the subject as impartial as possible. The more theoretical questions bearing on definite tectonic hypotheses are reserved for the following chapter and in the last chapter an attempt will be made to formulate a more embracing conception based on all the various lines of enquiry at hand.

Let us first consider the structural forms that are known, which might play a part in producing this relief.

In the first place there are the elevations formed by volcanic activity. Then there are blockfaulted positive and negative forms. Normal folding and thrusting might produce elevations and depressions. Finally extensive areas may have been either raised or sunk without rupture or folding, but only by a slight warping of the crust, possibly only along the margins of the area in question.

It is of great importance to realise the size and shape of the forms appearing on the chart. These are of a different order of size and intensity than the folds in a mountain system. The sections on the plates in this volume immediately show that the relief is of the same order as that of the land surface, discounting the details of the erosional surface of the latter [1]).

The topography of a mountain system, however, is only a very weak counterpart of the intensive relief of the structural planes that dip below and rise above the surface of the land. As the echo sounding sections of the sea bottom are similar in general forms to the land surface, it immediately follows that they do not show individual anticlines and synclines, comparable to those of most folded areas of the dry land.

There are three reasons for this and possibly there may be one more. *In the first place* the soundings were taken too wide apart for showing slight undulations of the bottom. The distance between the soundings was one to three kilometers. The distance from anticline to anticline would have to be several times this distance to appear clearly in a sounding section. Besides, a second section

---

[1]) On plate VIII a few sections of the land surface are shown, constructed from the international topographical map, scale 1: 1.000.000. On this scale no minor topographical details are represented, consequently the resulting sections can better be compared to echo sounding sections in which minor details also drop out.

parallel to the first would be needed at a small distance. A connection of the high and low points in the two sections would reveal the ridges and troughs. There are only very few points on the chart where the sounding sections are sufficiently close together to allow a distinct correlation of the smaller elements of the relief.

In the second place the nature of echo soundings renders it impossible to reach the bottom of narrow, deep troughs and the greater the distance below the surface the less the soundings will penetrate into a narrow trough (see fig. 11).

There is still a *third reason* for the apparant absence of folded forms on the sea bottom and that is the levelling influence of sediment. How great this influence is, depends not only on the age of the folding and the rate of the sedimentation, but also on the strength of currents that considerably increase the levelling influence of the sedimentation, by depositing most or all of the matter into the troughs. On the whole I am inclined to believe the influence of sedimentation is slight, as it works slowly and the submarine relief is probably not very ancient, geologically speaking. There are, however, no definite data at hand to substantiate this opinion.

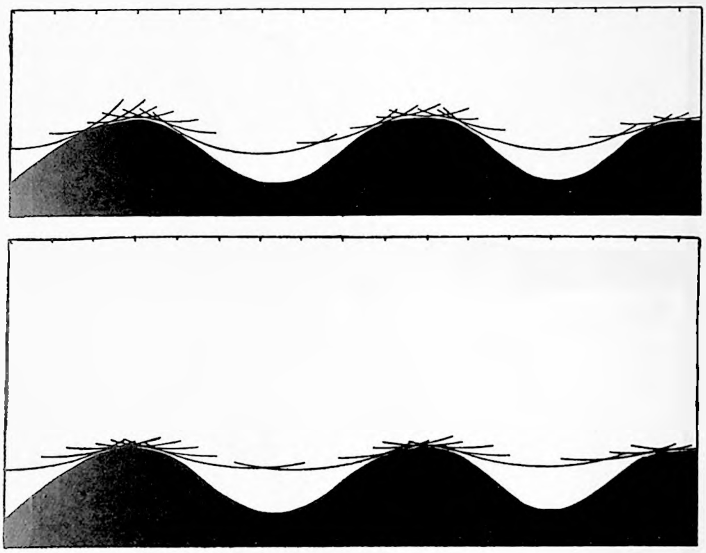


Fig. 11. Echo soundings over the same bottom at different depths. The deeper the bottom the flatter the echo sounding section.

Apart from the reasons given as to why a strong folding of the sea bottom could not appear clearly on the chart, *there is furthermore the possibility* that the surface of a folded region remains comparatively flat during folding. Many geologists hold, that folding is a characteristic of deeper seated layers of the earth's crust and that the surface over a folded area will only reveal slight undulations or blockfaulting. Although there are also reasons for doubting this opinion, the possibility cannot be gainsaid.

Illustration of these remarks is given by fig. 12. This sketch portrays a section through the Säntis range in Switzerland, submerged to a depth of about 3000 meters. The line a-a represents the surface section and b-b the result of a sounding section with intervals of 1000 m (= 5 minutes). The topographic forms are reduced to a simple, broad ridge and no indication could be detected in the sounding section of the intensive folding represented by the structural section c-c. If erosion had been prevented by the folding taking place below water, the topographic section would approximate to d-d, to which corresponds the sounding section e-e. Sedimentation would reduce the boldness of the topographic features and thus dull the sounding section still more.

The upshot of these remarks is, that the apparent absence of minor folded forms on the sea bottom is a negative characteristic. If the folded structure of the crust below a certain area of the sea bottom is probable, the absence of undulating forms in the chart is no definite proof, that the bottom is actually as flat as it is represented and the structural surface may be even more intricate still.

Let us now consider blockfaulted structures as they would appear on a chart. In chapter III we have noted the important fact, that great fault scarps have been found on the sea bottom. It might be concluded that blockfaulted structures must then appear with clearity on the chart.

Unfortunately this is not so. I have already pointed out that only faults with a very large downthrow can be proved by soundings and that when the fault is irregular or hades strongly it will not show clearly in the sounding section (compare fig. 13). To these drawbacks must be added that the proximity and the reliability of the echo soundings of the expedition, although considerable, was not sufficient to bring out all even of the larger scarps that were crossed on the cruise. Add to this the relatively large distance between the sounding sections and the levelling influence of sedimentation and it will be obvious that only a small fraction of the faults actually existing below the East Indian waters can be represented in the present data.

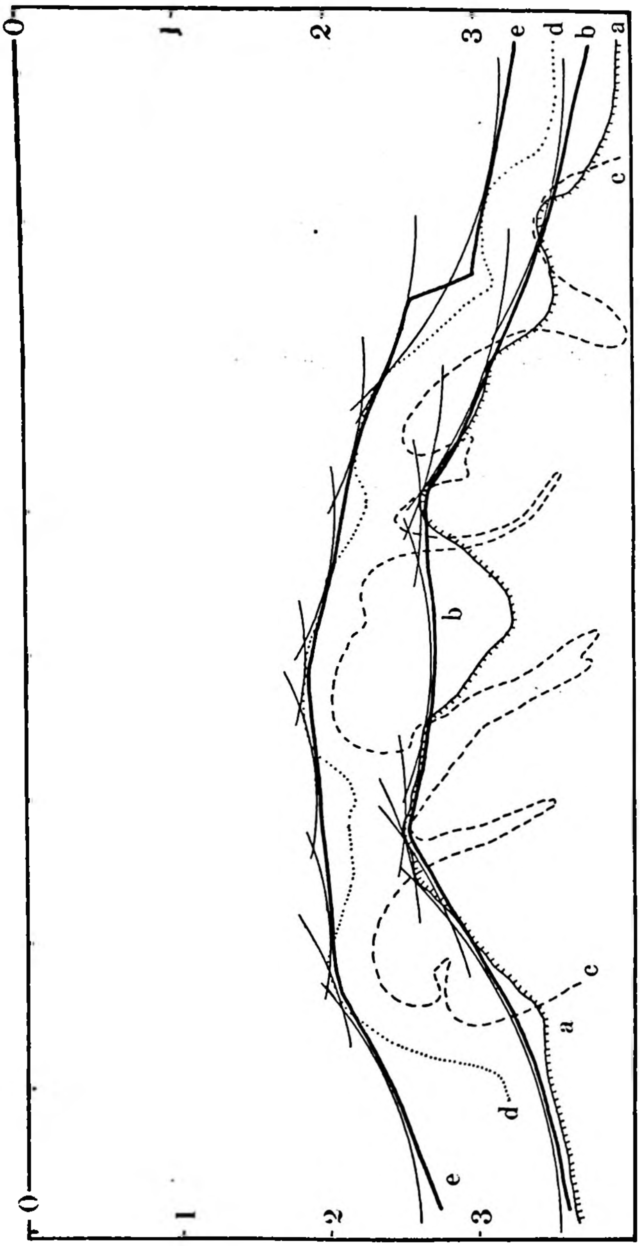


Fig. 12. Theoretical echo sounding section over the Säntis range in Switzerland (for explanation see text).

We will now study the morphological forms and seek to explain their structure. A distinction will be made between positive and negative forms, although the difference is frequently merely a question of relativity.

B. The positive forms (island arcs, banks, etc.).

The positive forms known to structural geology are: products of volcanic activity, anticlines, geanticlines and horsts. We will consider these forms separately.

Volcanic relief. In the production of the relief of the eastern part of the East Indies volcanic activity has acted a prominent part. The more ancient the period of activity, the less we are enabled to ascertain to what extent the present positive forms were actually built up by the production of volcanic matter during that period. There may lie equally thick ancient volcanic strata in the present negative forms as in the positive ones. The tertiary period of diastrophism that produced or started to produce the present configuration, probably acted in great measure independently of former

relief. It is to the tertiary — and above all — to the post-tertiary volcanic activity that we must therefore look to determine what positive forms were generated by eruptive activity.

Practically all volcanoes appear to lie on swells of the sea bottom, which exist independently of the extrusive materials. Goenoeng Api north of Wetar and possibly Oena Oena in the Gulf of Tomini are at present the only examples known of a volcano, which appears to rise up straight from the sea floor or very nearly so. All other volcanic islands are parasites on a tectonic structure.

On the other hand the relative amount of eruptive rocks is in many cases very considerable. If all young volcanic products were to be removed, the following islands would probably disappear or become greatly reduced in size: The entire Inner Banda Arc from Banda to Java, Ambon and the Oeliassers, the end of the northern arm of Celebes, the islands of the Sangihe Arc, Halmahera and surrounding areas. To the direct reduction caused by the removal of volcanic rocks, we must add the decrease in thickness of the non-volcanic sediments that were thickened by the addition of waste from the eroding volcanoes, or those sediments that were formed by organisms dependent on the reduction of depth through volcanic extrusions, such as coral reefs, and all non-planktonic shallow water animals and plants. It is as yet impossible to ascertain the extent of the reductions that would thus be produced. The Inner Banda Arc from Wetar to Banda would be reduced to a low ridge on the sea bottom at a depth of 3000—4000 meters. The Sangihe Arc on the other hand would still rise up to a few hundred meters below the surface and the same probably applies to the other parts specified above.

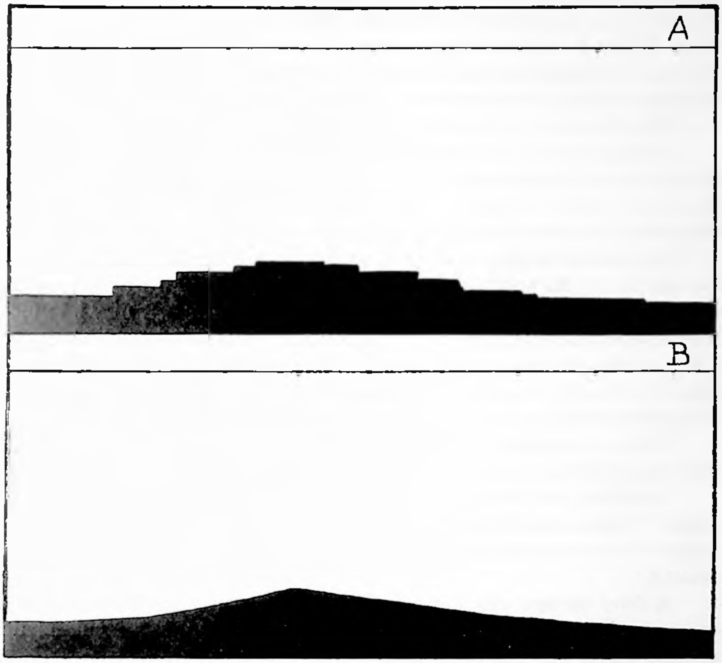


Fig. 13. Echo sounding section of block-faulted mountain. A Actual section, B Section as it would be found from the echo soundings at the indicated spots.

This conclusion, drawn from the morphology, is substantiated by the observation that, between Pantar and Damar and in the Banda Group, elevated reef rocks are known to occur up to heights of several hundreds of meters, whilst the marine tertiary strata of the islands from Java to Flores also attain considerable altitudes. The elevation thus proved, of itself indicates the existence of a geanticline as distinct from the volcanic relief.

In considering the relief formed by diastrophism, we must therefore bear in mind the fact that the islands and arcs in which tertiary and particularly quaternairy volcanoes play a part, are of considerably lesser importance than they would seem at present to be.

Blockfaulted horsts. We showed above why there must be several times the number of faults on the sea bottom than will ever be found by echo sounding, and that the number of faults already observed can constitute but a fraction of the total number that actually exist. A horst can be bounded by a series of step-faults, which each in itself have only a small down throw, and such a structure would appear as a gradually graded elevation on a sounding section (see fig. 13). To these difficulties

must be added, that a submarine scarp could also be produced by a flexure or a sharply bent, asymmetrical anticline. Finally it must be borne in mind, that isolated faults occur in folded regions and that folded regions are sometimes blockfaulted, subsequent to the folding. From this it follows, that on the sounding sections solely, we can scarcely expect to be able to distinguish all or even the majority of faulted elevations from those formed without — or mainly without — rupture of the crust. Albeit that our insight into the structure of the submarine elevations must consequently still remain very superficial, there are some factors that may assist us.

In the first place the structure of islands emerging from the sea may suggest the faulted nature of the region. The best example of this case, so far discovered, is found in the Toekangbesi Group. The detailed treatment of this group has already been given in part 2 of the geological results of the Snellius Expedition (Vol V). Hetzel (bibl. 54) deduced from the examination of the islands, that the structure was dominated by faulting. My own observations were in complete agreement with this view. In the echo sounding sections of this region several scarps were discovered. In this case, therefore, the bathymetrical data form a welcome addition to the structural knowledge of the group, assisting to demonstrate the essentially faulted nature of the relief (see Pl. VIII, section 93 and Pl. IX).

We will revert to the subject of faults in a later section.

A second method of gaining an insight into the structure of the sea bottom is by ascertaining the horizontal projection of the elevations and seeing whether these bear more resemblance to faulted or to folded regions. The former are characterized by an abrupt, rectangular ground plan, the latter by curved and tailing off forms.

Here again, we must be careful not to overestimate the completeness of our data. In constructing the chart from the soundings, the only possible method was to draw flowing curves between sections and islands and isolated soundings. The curves, however, may be much less smooth, or even very abruptly bent here and there. If the number of soundings were increased several times, whereby a much greater accuracy of the chart could be attained, many of the present flowing, rounded forms might be found to be sharp and angular in actual fact. The result would be to add much weight to the interpretation of the forms as being blockfaulted horsts.

So far, the general impression to be gained from the chart is, that the major forms, at least, are bent gradually and evenly; therefore, that faulting is not the principal cause of the relief.

The Soela Islands are the only group in which the general form reminds us strongly of faulted horsts. There are admittedly many coasts of other islands, which suggest a faulted origin. Some cases will be noted when dealing with the negative forms to which they appear to be genetically related.

A third method calculated to help in differentiating between faulted and folded structures is to estimate the degree and the nature of continuity from one culmination to another. This will be treated under the next heading.

Anticlines and geanticlines. After what has been previously set forth it will be clear, that normal anticlines and synclines are too small to be clearly recognised, as yet, in the sounding sections. Even the smallest undulations appearing on the chart measure many kilometers form crest to crest.

Practically all the positive forms we observe are of the magnitude of geanticlines. Although reasons may exist for believing that they represent elevations produced by compressive folding, *the sections* could have been caused by vertical movements, with or without the production of faults.

In the Toekangbesi Islands it was shown by Hetzel and myself that the folding ended prior to the emerging, for the elevated reef terraces are horizontal and flat.

In my opinion it is of great import, that virtually all elevated reef terraces are un-warped and that in the exceptional cases where the terraces are tilted, they nevertheless retain the flat surface in a very gently dipping position. This does not go to prove that folding is not in progress at greater depth in the manner assumed by Molengraaff and Brouwer, but it does serve to show that the parts of the geanticlines which emerge above sea level are not being folded into normal anticlines and synclines in the upper layers of the crust. It is reasonable to conclude that the same absence of normal folding must be a feature of the upper layers of the submerged parts of the geanticlines. This is to say that the absence of folded forms in the present chart will likely remain after a more detailed examination of the morphology of the sea bottom has been accomplished.

We will not here enter into the question, of whether folding is in progress in deeper strata

during this present period, for it is sufficient for us to know, that the superficial layers are not undergoing normal folding. It is a different matter respecting whether the obvious, recent elevation is due to faulting or a warping of the crust in broad undulations (with or without more intensive compression in the lower layers of the crust). The existing data justify the conclusion that folding is not taking place in the superficial layers, but the great breadth of the geanticlines, measurable in dozens of kilometers, renders it impracticable to solve the problem of whether warping is in progress, by means of examination of the rough diagrams of elevated reef terraces. Strictly accurate measurements of the shape of the terraces would be required to solve this problem to satisfaction. In the meantime we must follow other lines of thought in order to enable us to arrive at an opinion on these matters.

The principal reasons supporting the belief that compressive folding constituted an important factor in producing the positive forms of the present relief are the following.

1) The regular alternation of a series of positive and negative forms of considerable length and corresponding breadth and height (or depth). In blockfaulted regions the number of major horsts is mostly restricted, the height of these both cross-wise and length-wise is highly variable, likewise their breadth. This line of investigation is vague, partly on account of the insufficient data and will therefore not be emphasized by giving examples.

2) The fact that the strata of all islands which emerge are found to be folded in lesser or greater degree.

It is true, as we have seen above, that the folding evidently ceased some time before emerging, either entirely or only at the surface. This does not alter the fact, however, that folding was (or still is) a very important factor in the production of the present elevations. It is highly improbable that the entire region of the eastern part of the East Indies was folded in a former period and that vertical forces of the present period carried portions of this structure to the surface in a chaotic design.

The older structures are essentially parallel to the present relief, (although there are minor deviations) and consequently the previous compressive folding was restricted to, or at least concentrated in, the present geanticlines (and the master-synclines accompanying them?) The folding may have gone downwards to a certain extent, but there is no reason for supposing that this folding failed to produce a certain measure of elevation. Hence, the conclusion that the present positive forms owe their elevation to some, as yet unknown, extent to former compressive folding.

3) The manner in which many of the geanticlines alternate with one another where they join, is suggestive of folded, not of faulted forms; in other cases, the reverse explanation would appear to be the most reasonable. The data upon which the previous chart was constructed, were so scarce, that the geanticlines were drawn out in rounded forms and the separate parts connected by simple shapes. We have now to reconsider these crucial parts on the evidence which they may give. With a view to their importance, also from an oceanographical point of view, many of these connections were sounded in greater detail, so that the data are relatively complete.

The straits between the adjacent islands, on a geanticline, in localities where the rows of islands and the geanticline form a straight or only slightly curved line must be explained by faulting (Molengraaff bibl. 85, p. 290). On the volcanic arcs the straits are probably not of tectonic origin atall, but simply unfilled gaps that are kept open to a certain extent by tidal currents.

The relation between Soemba and the Bali-Flores arc is no direct connection. Soemba dips down to the west alongside the arc. Such a relation is not typical either of faulting or of folding.

The position of Soela Sanana at right angles to the Taliaboe-Mangole row, which it fits onto quite abruptly, apparently down to considerable depths, distinctly favours the opinion that faulting produced the relief here. The steep slopes from all three islands down to the flat bottom of the northern Banda Basin point to the same conclusion. In this connection it is of importance to remember that the geological history of these islands, so far as it is yet known, was not characterized by strongly developed, geosynclinal conditions while the young diastrophism was not intensive.

The submarine shapes of the straits at both sides of Sawoe are not bold but they are nevertheless strongly reminiscent of faulted forms. There are reasons for assuming, however, that this is the superficial expression of deeper seated folding. The Manipa Strait will be commented on later.

There are a number of connections of quite a different nature that are easily reconcilable with the folding hypothesis, but are difficult to account for by faulting. These are cases in which two

parts of the major geanticline overlap side by side, thus establishing an interchanging relation: an „en-echelon" structure.

This interchanging relation is frequently to be observed in folded structures. Very fine examples have been described from the Swiss Jura, for instance. In blockfaulted structures they are absent, to the best of my knowledge. This difference is easily explained by the nature of the movements. When a portion of the earth's crust is compressed horizontally the foreshortening is concentrated first in one anticline. Further along the strike a second anticline commences and increases in importance and the greater the amount of foreshorting it represents, the less remains for the original anticline, which, as a result, slowly fades away, pitching down along the side of the rising, second anticline (fig. 14).

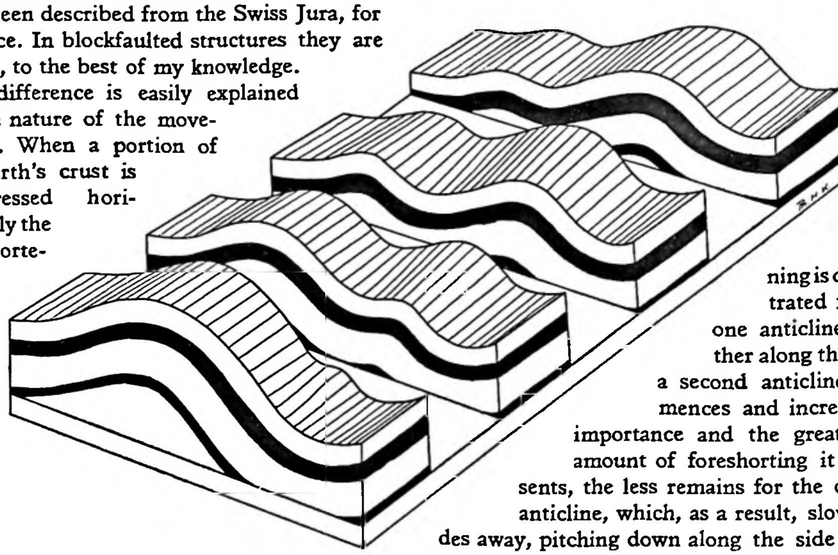


Fig. 14. Block diagram showing the interchange of two anticlines.

In some cases, the first anticline can regain predominance still further along the strike, in other cases a third anticline will cause the second to undergo reduction in the same relative position as existed between the two former (fig. 15). This latter case represents the true „en-echelon" structure. It is probably developed when the compressive force acts obliquely to the mobile belt which is undergoing the folding. Now, in the case of blockfaulting the movements are principally vertical. Whether the primary forces are chiefly horizontal or vertical, need not concern us here; the

Fig. 15. Ground plan of interchanging anticlines. A true en-èchelon arrangement, B with reappearing anticlines.

resultant movements are upwards or downwards. This being so, the generation along the strike of a second horst would not influence the first horst gradually and cause it to lose by degrees in importance. It would either come to an abrupt termination, or continue for an arbitrary distance.

An „en-echelon" relation of positive forms is therefore a very strong indication of compressive folding. The clearer and the more often repeated this relation is, the greater certainty we have of dealing with an undulation caused — not by faulting — but by folding.

Suess (bibl. 110) and Tokuda (bibl. 115) already pointed to the frequency of the „en-echelon" structure in island arcs of the western Pacific; however, without emphasizing the conclusion, that it is such strong evidence against the faulted structure of the geanticline under consideration. In my opinion this interchanging relation is tantamount to proof, that the island arcs characterized by it, were produced mainly by compression and that theories based on stretching („Zerrungshypothesen")

are not applicable to these structures. It has been pointed out already, that this folding may find expression at the surface in the form of faults.

The „en-echelon" structure is to be met with in several parts of the East Indian arcs. Clear examples are found on the Outer Banda Arc in the following places: between Timor and the Leti-Moa Group, between the latter and the Sermata Islands, between these and the Babar Group (see fig. 43). Less clearly the same structure may be found between Sawoe and both the neighbouring Soemba and Roti, between the Babar- and the Tanimbar Islands, and on the Ambalaoe-Ambon Arc.

As regards the Inner Banda Arc, the relations are less defined and more soundings are required before a definite opinion can be formed. The ridges to the south of Boeroe, particularly the Siboga Ridges and those further west joining on to the Toekangbesi Group afford several examples of the „en-echelon" arrangement.

The connection between Boeroe and Soela Sanana may belong to the same class, but more soundings to the west and northwest of Boeroe are essential for establishing this conclusively.

The connection between the Soela Islands, Obi and Misool is built entirely according to the „en-echelon" pattern.

The central complex of ridges in the Molukken Sea is too complicated for detailed analysis on the evidence supplied by the present sounding sections. Between Majoe and Tifore there is an interchange and in the case of several other culminations it is at least feasible. It appears likely that further investigation will reveal a pattern in which the „en-echelon" arrangement plays an important part. The plan of the Talaud- and Nenoesa Group is curious, but it is not a clear example of the „en-echelon" structure; the Snellius Ridge, on the other hand, appears to indicate interchange of the culminating banks.

To sum up: the „en-echelon" pattern is to be found on the greater part of the Outer Banda Arc, the Siboga- and Luymes Ridges, the connection between the Soela Islands and Misool and the central ridges of the Molukken Sea. We will return to the distribution of this type of connection when reviewing the region as a whole.

It was pointed out above, that the horizontal projection of the positive forms is generally curved and that this shape is characteristic of folded forms, whereas it is absent in blockfaulted regions. With the exception of the Soela Islands all the geanticlines are bent in greater or lesser degree.

C. *The negative forms (deep-sea troughs, etc).*

When we attempt to explain the formation of the negative shapes of the relief, we have much less data to go by than in the case of the positive forms. Not only is the shape of the latter much better known on account of the shallower depth — or even emergence here and there —, but the geological structure and lithological composition of the islands are very important aids to the unravelling of the history and manner of formation of the entire structure on which these islands are placed. This being the case it is curious to mark the number of opinions and theories which have been submitted on the formation of the negative forms, particularly of deep-sea troughs. The new soundings have at any rate provided us with a sounder morphological basis on which to found an opinion on the generation of the negative forms. Still we must not attach too much weight to these opinions, for the reason that morphology is only anatomy of the surface and that the structure of the deeper lying strata can so far only be conjectured.

Much of what has been expressed concerning the positive forms applies also to the negative shapes. The difference between the two is, partly, simply a question of relativity. An-

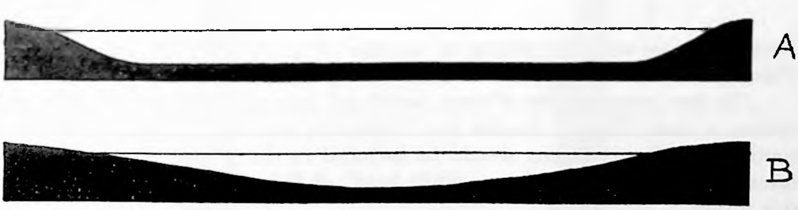


Fig. 16. Ideal sections (heights exaggerated), A of the type of the Celebes Sea, B of the type of the Timor Trough.

ticlines side by side imply the existence of synclines and the same applies to horsts and graben. Possibly the most important difference is that there are no positive equivalents to the deep and broad basins, of which the Celebes Sea forms the best example.

The negative forms can be subdivided into two main types and each of these into two respective groups.

The first type of basin is characterized by a flat and horizontal bottom with relatively steep sides.

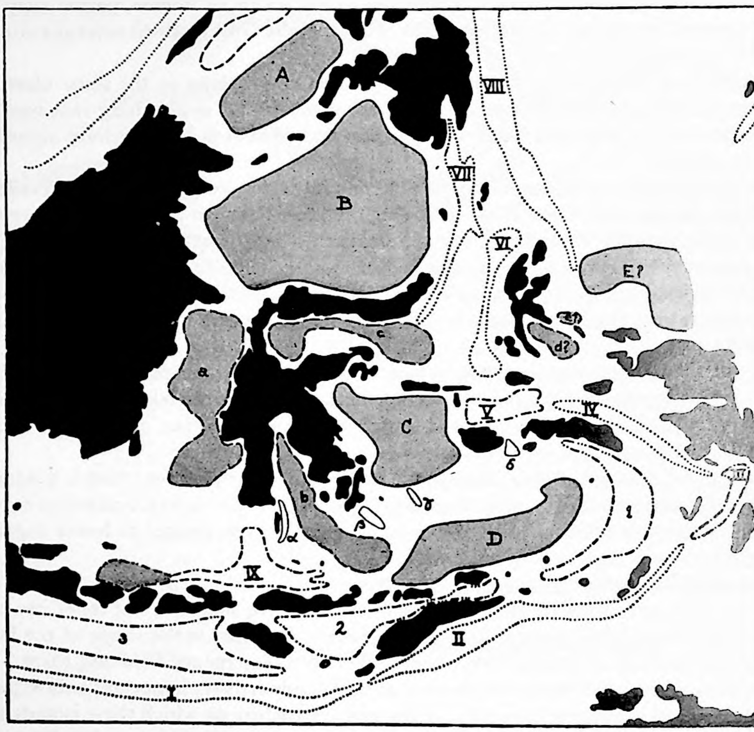


Fig. 17. The basins and troughs of the eastern part of the Netherlands East Indies.
A—D first group of basins.
a—c second group of basins.
1—3 third group of basins.
I—IX deep-sea troughs.
α—δ fifth group of basins.

Those examples which are oblong do not taper off gradually or form long festoons, but end abruptly. This type of basin comprises two distinctive groups.

The first group consists of large, broad and deep basins with steep sides and a flat, horizontal, bottom at about 5000 m. Besides the Celebes Sea, possibly the Sulu Sea, the northwestern and the southeastern Banda Basins should be included in this group. The single echo sounding section through the basin north of the „bird's head" of New Guinea indicates that it may also belong to this group.

The second group possesses the same flat, horizontal bottom with fairly steep sides but is not so deep (about 2000 m) and is oblong in shape. One end generally breaks off abruptly and sometimes

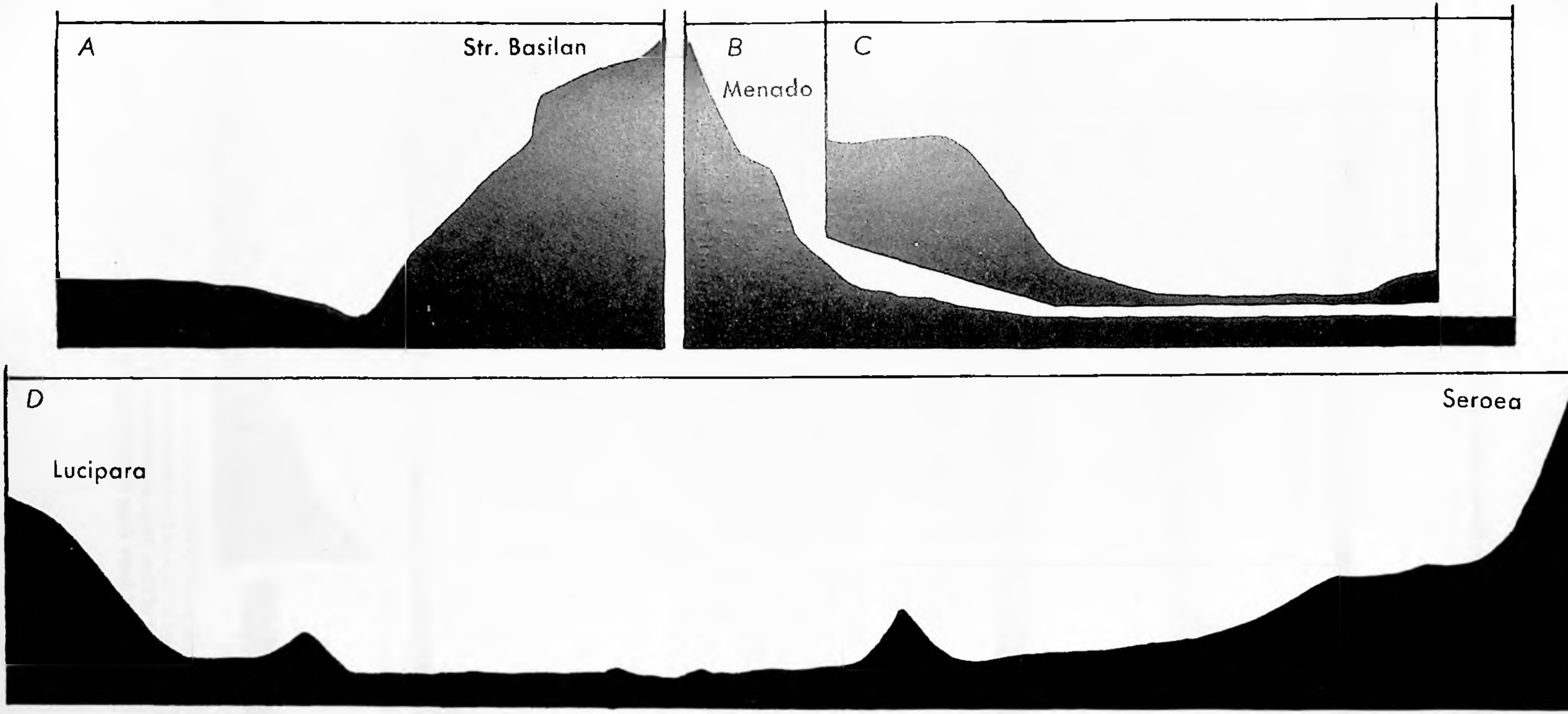


Fig. 18. Bottom sections, horizontal scale 1: 1.000.000, vertical scale 1: 100.000. Basins of the first group (1st type).
A Sulu Sea from Basilan Strait to northwest.
B Celebes Sea from Menado to northwest.
C Basin north of „bird's head" of New Guinea.
D Southeast Banda Sea from Lucipara Islands to Seroea.

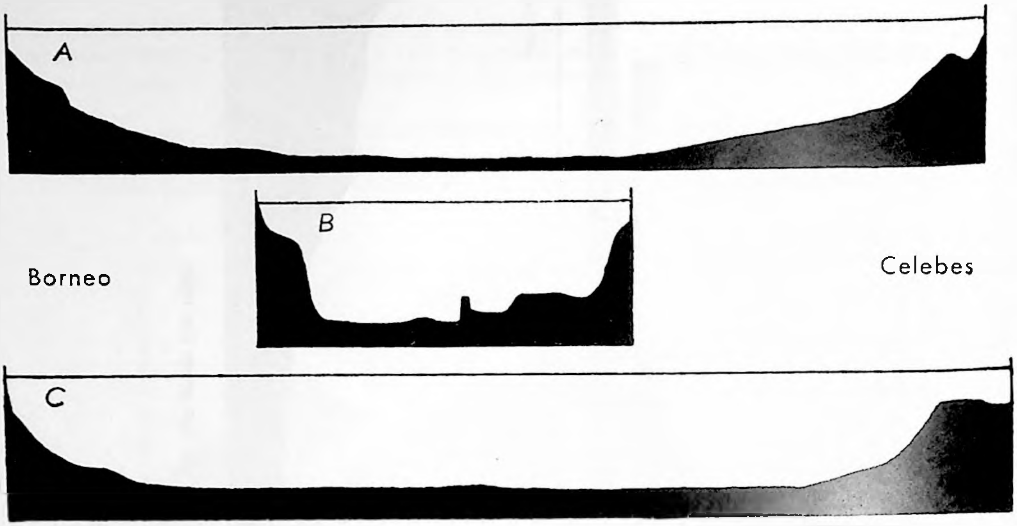


Fig. 19. Bottom sections, horizontal scale 1: 1.000.000, vertical scale 1: 100.000 Basins of the second group (1st type).
A Northern part of Makassar Strait (west on the left).
B Central, narrow part of Makassar Strait.
C Southern part of Makassar Strait.

the breadth varies abruptly. Examples are: the Makassar Strait, the Gulf of Bone and the Gulf of Tomini west of the 124th meridian and possibly the Halmahera Sea and the deep north of Bali and Lombok. The Sawoe Sea might be placed in this group, but it is obviously connected with the Weber Deep and is therefore counted in the third group. The Gulf of Bone is linked with the southern Banda Basin and the Makassar Strait with the Celebes Sea. Although there is some indication of a divide the connection supports our classification which unites the two groups in one main type (see fig. 16).

The second type of basin is oblong with tapering ends, frequently forming festoons, the depth

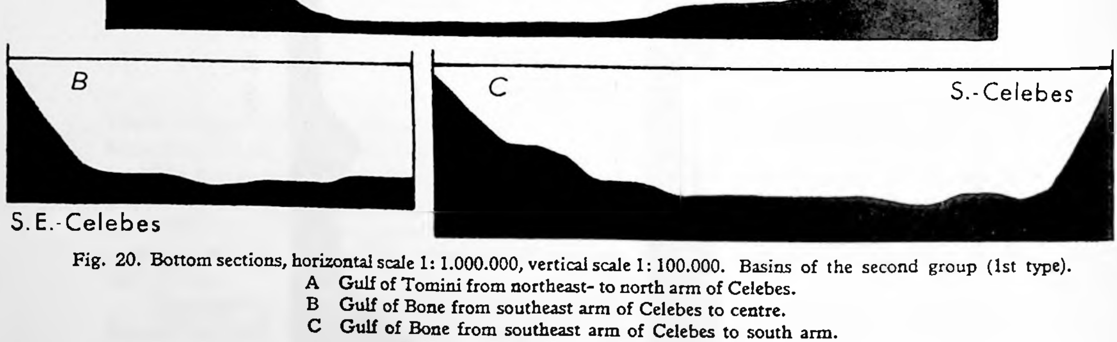


Fig. 20. Bottom sections, horizontal scale 1: 1.000.000, vertical scale 1: 100.000. Basins of the second group (1st type).
A Gulf of Tomini from northeast- to north arm of Celebes.
B Gulf of Bone from southeast arm of Celebes to centre.
C Gulf of Bone from southeast arm of Celebes to south arm.

varies considerably, the ground plan is gently curved, the breadth varying gently and within fairly narrow bounds. The most typical examples show a shallow synclinal cross-section. We can again make a subdivision in a third and a fourth group of basins.

The third group is also characterized by steep sides and a flat bottom in profile, but the latter is not horizontal, as the trough-line varies in depth along the centre of the oblong trough. The Weber Deep with its continuation: the Sawoe Sea, is the best known representative, but the other basins, along the coast of Java and Sumatra, should also be counted. Their shapes are very imperfectly known. The depth varies from 1000 to over 7000 m. These basins are placed between the Banda Arcs and their continuations to the west. This group is more or less transitional between the former type and the following group.

The fourth group of negative forms are oblong having varying depth whereby the trough line swings upwards and downwards. The cross-section is that of a broad and shallow syncline, sloping down gradually to the centre and immediately commencing to rise to the opposite side (see fig. 16). The breadth changes only slightly and gradually. The ground plan is usually curved and they taper towards the ends, or are strung together in uninterrupted continuity. The representatives are: the Java Trough, Timor-Ceram Trough, the troughs of the Molukken Sea, the Mindanao Trough and possibly the Flores Sea. The Aroe Trough is more box-like in cross-section and probably ends abruptly against the south coast of New Guinea. It constitutes a transition to the second or first group and the same may also be said of the Boeroe Basin.

In the fifth and last group we can incorporate all the small negative forms that are still remaining, albeit that it is improbable that they belong to one another genetically. These are: the Salajar Trough, the Manipa Strait and the troughs at both sides of the Toekangbesi plateau.

Since the larger volcanic forms are invariably positive, we must explain the negative forms by faulting, folding or warping.

Several scientists have advanced an opinion on the mode of formation of deep-sea troughs in general. Jensen thought they were of a faulted nature (bibl. 59). Supan (bibl. 111) and Marshall (bibl. 77) have attributed their formation to compressive folding. Suess (bibl. 110, Bd. III, 2, p. 336 and 581) held that they had been overthrust by the land and weighted down. Wegener (bibl. 127) considers them to be the „wake" behind moving continental masses (see also p. 98 etc.).

Taber (bibl. 112, 113) and Hess (bibl. 52, 53) believe that the Bartlett Trough in the West Indies was formed by faulting. The structure of the surrounding islands and the accurate echo sounding sections are the arguments put forward by these authors.

It is not probable that all the negative forms were produced by the same agents. We must therefore consider our groups separately.

The first group. Everything points to the faulted nature of these formations. The considerable size, the box-like section, the equi-dimensional shape, the absence of complications of the floor are so many features opposing a folded structure. As these great basins are all characterized by a strong positive anomaly of the gravity, they must apparently possess a tendency to sink. This lends support to their faulted nature. The view is also tenable, that they represent primary features of the earth's crust, as permanent as the oceans, and thus, that they do not call for a separate explanation. It might also be urged that if they were mechanically foundered land areas, they ought to show a strong negative anomaly, because the sial forced away sima. This objection to the hypothesis of depression cannot carry much weight.

The science of geophysics is still in its infancy and the fact that as yet it can give no final explanation of the foundering of extensive areas, is not proof against the feasibility of this assumption. There is still ample scope for new hypotheses for explaining such a happening as, for example, condensation or convection in the sima. Vening Meinesz for instance, advocates the latter possibility to account for the formation of the basins by subsidence and for the positive anomaly of gravity of all ocean basins (bibl. 120). Moreover, some explanations must be given in any case for the present positive anomaly, which in itself corresponds to a further sinking en-block of more than 1000 m. The land-locked (= „sial-locked") position of the basin is decidedly in favour of the theory of foundered crustal blocks. Umbgrove, in his discussion of the relations between gravity field and geological build of the islands, offers further evidence, pointing in this direction (bibl. 119).

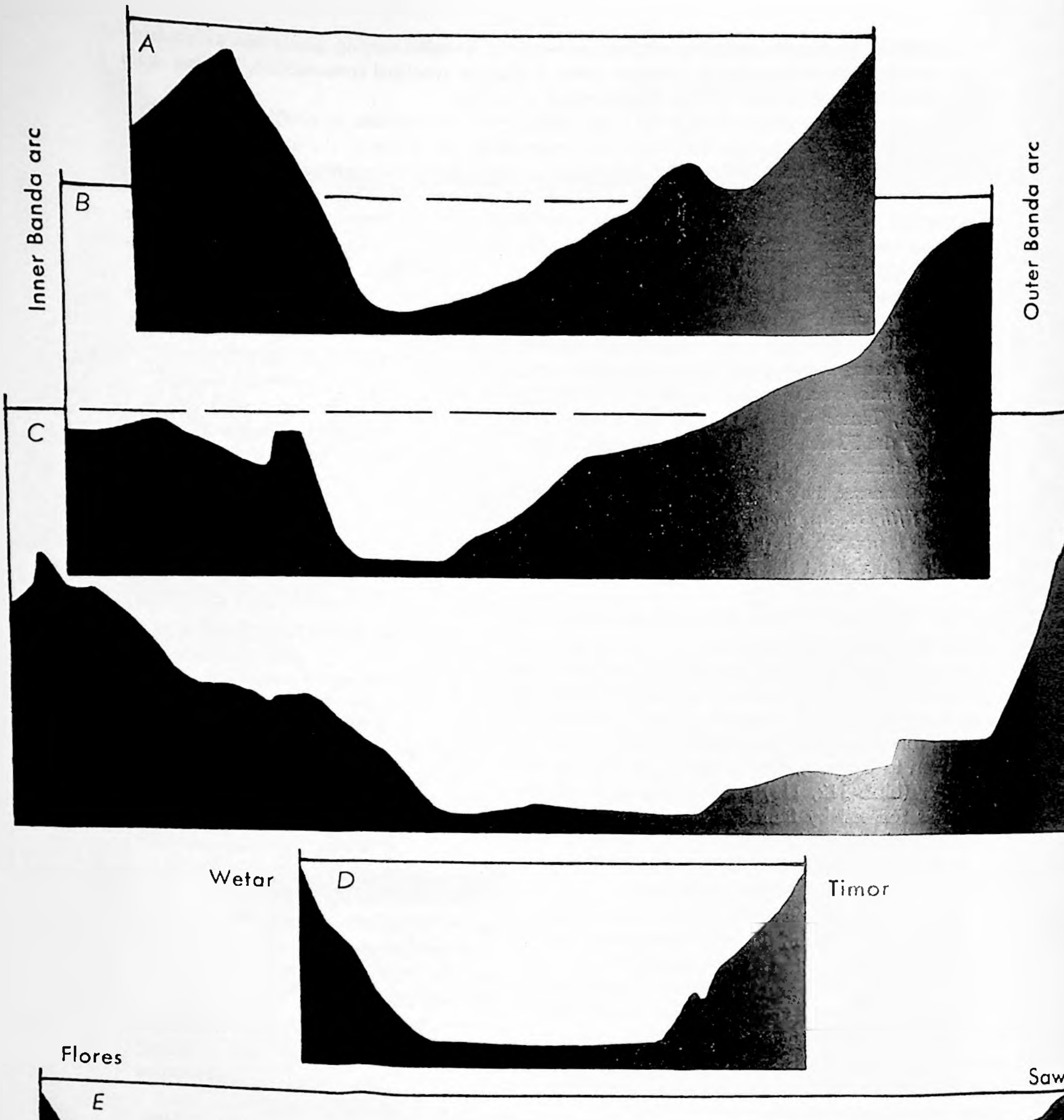


Fig. 21. Bottom sections, horizontal scale 1: 1.000.000, vertical scale 1: 100.000 Troughs of the third group (2nd type).

A Weber Deep from Banda Plateau to south point of Ceram (on the right).
B Weber Deep from north of Seroea to the Kai Islands (on the right).
C Weber Deep from Seroea to north of Tanimbar Islands (on the right).
D Weber Deep from east point of Wetar to Timor.
E Sawoe Sea from Ende to Sawoe.

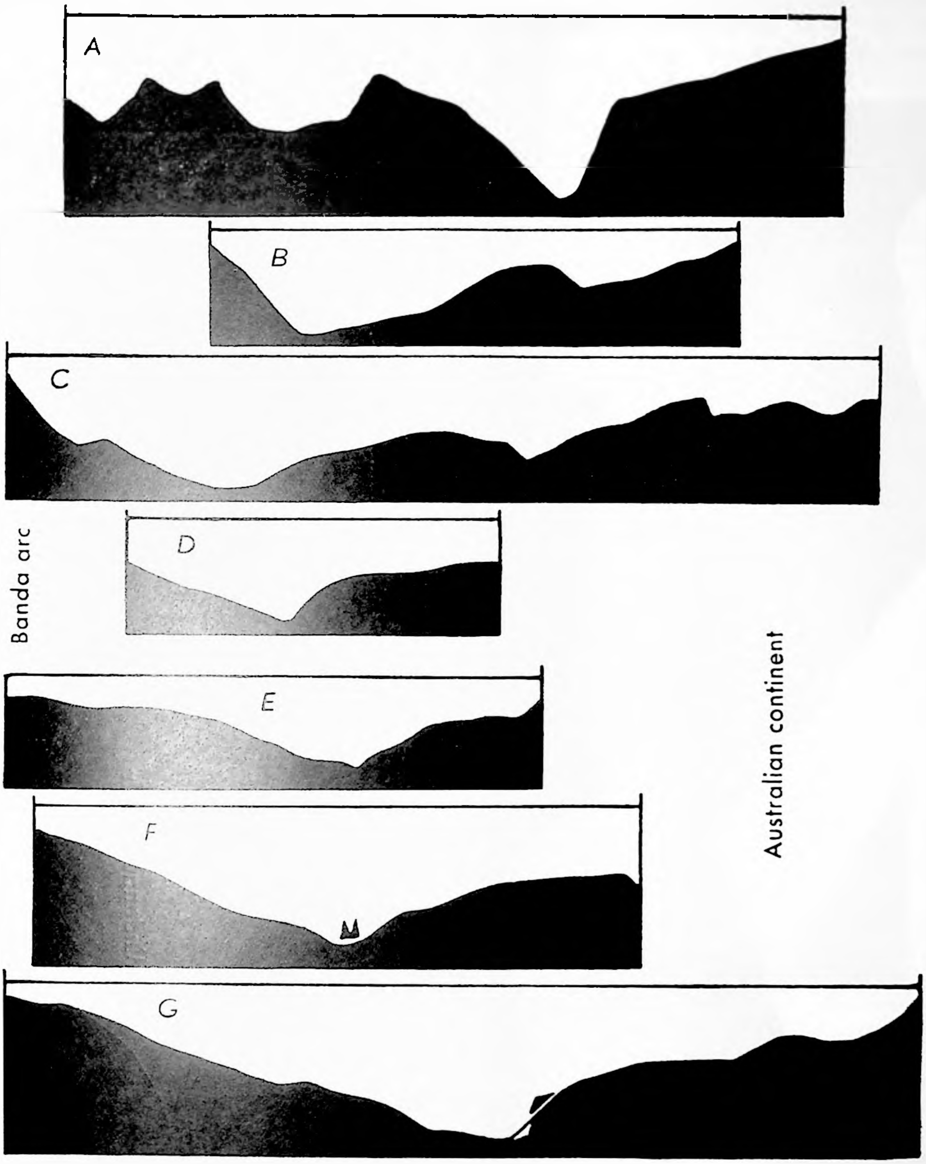


Fig. 22. Bottom sections, horizontal scale 1 : 1.000.000, vertical scale 1 : 100.000. Troughs of the fourth group (2nd type).

A Ceram Trough from middle of Ceram to Obi-Misool Ridge (on the left).
B Ceram Trough from Boela to Gulf of Mac Cluer (on the left).
C Ceram Trough from New Guinea to Manawoka (on the left).
D Timor Trough from north of Jamdena to the east (on the left).
E Timor Trough from Jamdena to the east (on the left).
F Timor Trough from Babar to the south (on the left).
G Timor Trough from middle of Timor to the southeast (on the left).

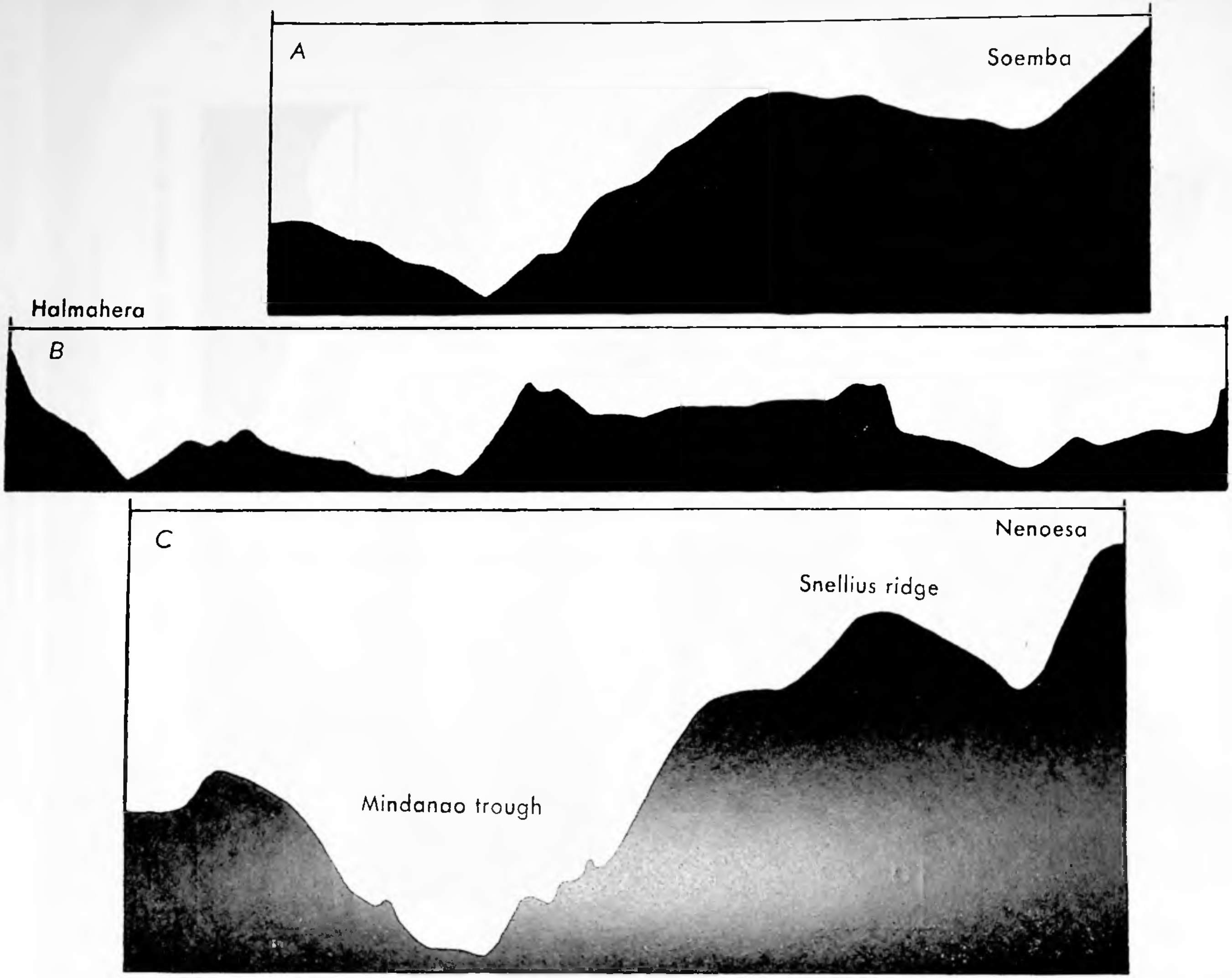


Fig. 23. Bottom sections, horizontal scale 1 : 1.000.000, vertical scale 1 : 100.000. Troughs of the fourth group (2nd type).
A Java Trough from Soemba to the south. B Molukken Sea from north of Halmahera to Biaro. C Mindanao Trough from the Nenoesa Islands to the east.

The notion of a down faulted area must not be taken too literally. It is possible that marginal flexures or stepped faults play a much more prominent part than actual faults. At any rate, the sides of the basins are not nearly steep enough for representing fault scarps, because the general slope is only a few degrees. In mentioning of a down faulted area, it is only intended to emphasize the importance of the vertical movement, attributed to a vertical regional force, in contrast to secondary vertical forces, produced by compression acting progressively from two sides towards a common centre. In the former case, the maximum effect is attained close to the borders and does not progress further towards the centre. In the latter case, there is a trough line corresponding to the central belt where a maximum is reached gradually from two opposite sides.

The second group. Here, as well, there are many points in favour of faulting. The flat, horizontal bottom and steep sides are obvious and the ground plan — at any rate of the Makassar Strait — points in the same direction. If we take note of the abrupt ending, both in the north and in the south, and also the sudden narrowing in the centre and the straight course of the structure, as a whole, we are at a loss to find an explanation, other than by faulting. The gravimetrical data support this contention.

Just as with the former group, down faulting is not to be taken literally.

Thus we arrive at the conclusion, that both groups belonging to the first type were formed by a primary vertical and regional force.

The third group. An explanation of these troughs is more difficult. The sections appear to be of a faulted nature, but the variation of depth, the complications around Kisar and the strongly curved ground plan, are contradictory to this view. The gravimetrical character is not constant, for the Weber Deep is strongly positive, whilst the Sawoe Sea is crossed by the belt of negative anomalies. For the present we must refrain from offering any opinion.

The fourth group. These troughs appear as the negative equivalents of the geanticlines, formed by compression. All are curved in ground plan, the section is weakly synclinal. The breadth is more or less proportionate to the depth, but varies less. They are sometimes divided by a central ridge, as in the Ceram Sea. Furthermore, they are linked with the geanticlinal forms, which are believed to have been formed by compression, having regard to the morphological form, the structure of the islands, and the gravimetrical data. They are all situated close to the belt of strong negative anomalies. All these observations converge to the same conclusion: the negative forms of this group were formed in conjunction with the accompanying geanticlines, by compression. They are synclines, built on the same grand plan as the geanticlines, in consequence of horizontal compressive forces.

Two points must be noted. The Flores Sea probably belongs to this group, from a mor-

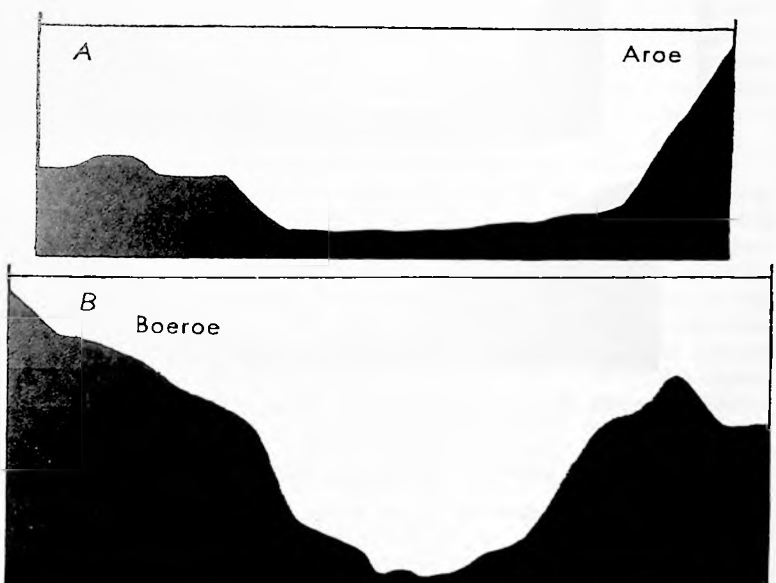


Fig. 24. Bottom sections, horizontal scale 1: 1.000.000, vertical scale 1: 100.000.
A Aroe Trough from Dobo to the northwest.
B Boeroe Trough from middle of Boeroe to the north northeast.

phological point of view. On the other hand, there are some respects in which the Flores Sea constitutes a transition to the first type. In the centre the breadth suddenly increases considerably, the greatest depth is highly constant and just over 5000 m, the position on the inner side of the Banda Arcs,

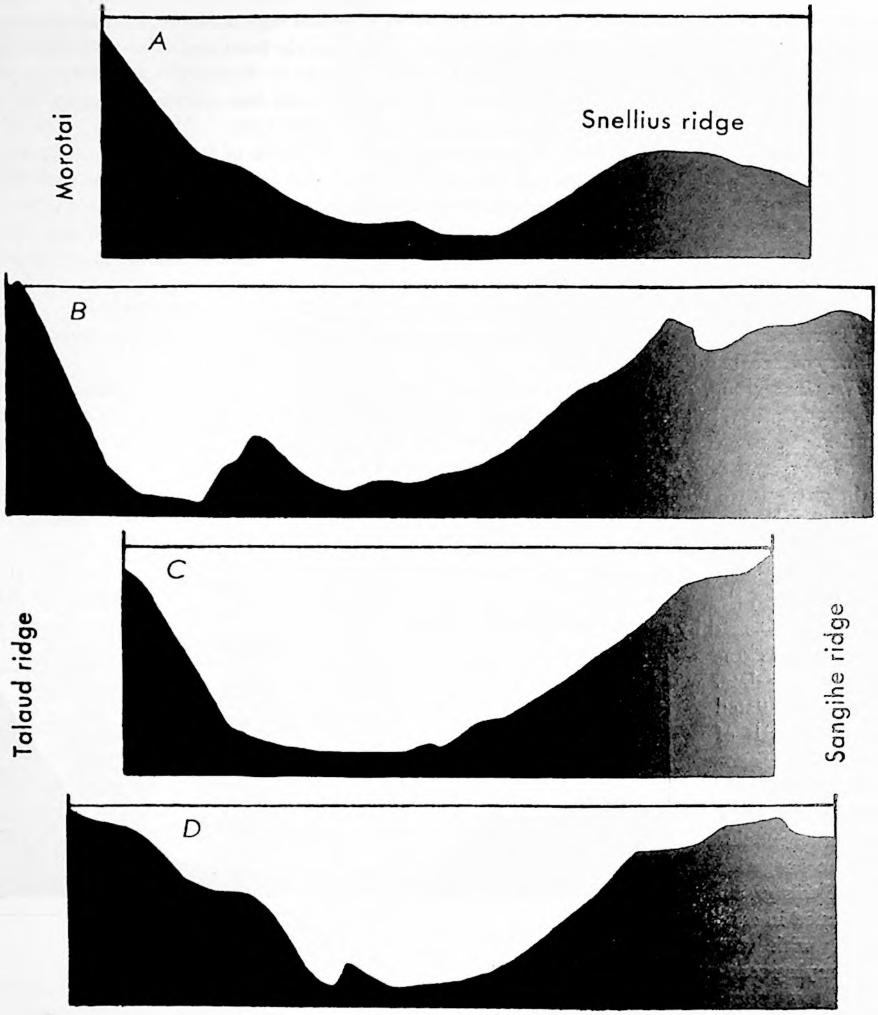


Fig. 25. Bottom sections, horizontal scale 1: 1.000.000, vertical scale 1: 100.000.
A Morotai Trough from Morotai to the Snellius Ridge.
B Talaud Trough from Miangas to the west (on the right).
C Talaud Trough from north point of Talaud to Kawio Plateau (on the right).
D Talaud Trough from middle of Talaud to north of Sangihe (on the right).

a positive anomaly of the gravity. It is further linked with the basin north of Bali, that probably belongs in the second group.

Then the deep Aroe Trough is of slightly different shape, with steep sides, a flat bottom and ending so far as is known, abruptly against New Guinea in the north. The Boeroe Trough is similar

and possesses a very strong positive anomaly. The possibility exists that these three troughs were formed in part by the influence of other stresses than the more typical examples of this group. In this connection it is of importance to note that the Aroe Trough lies further removed from the line of Vening Meinesz than any other part of the Timor-Ceram Trough line and that the Boeroe Trough also lies at a point where the line of troughs deviates from the main line of negative anomalies of the gravity. The Talaud-, Morotai- and Batjan Basins are all instances of the same relationship in a less pronounced degree, in which there is reason for believing the first type of forms is approached. These cases may be considered as transitions to the still more pronounced deviation and differentiation, as presented by the trough on the western side of the Molukken Sea, where it bends away to the broad, flat-bottomed Gulf of Tomini.

The fifth group. We must treat the members of this group, separately, because they are not all alike.

The Salajar Trough was formed by faulting, according to Verbeek (bibl. 121). There are several features in favour of this view. The sides, especially of the western slope, are tremendously steep, while the elevated reef terraces of Salajar, are flat, but hade away slightly from the trough and the course of the trough is straight. The bottom, however, is not flat but synclinal in cross-section, with a complication in the south.

The troughs at both sides of the Toekangbesi Group were probably formed at the same time with the plateau on which the islands and atolls are placed. There are no fault scarps except those close to Binongko and the bottom is synclinal in cross-section. Warping of the crust by the same compression that acted in the formation of the group, appears to be the most likely agent for explaining these troughs.

The deep basin in the Manipa Strait is probably less complicated, structurally, than would appear from the chart. The steep cone in the south is almost for certain a volcano, so that the actual basin is three cornered in shape, having steep sides and a flat, horizontal bottom.

It seems most likely that it is a faulted graben, closed at the south by a ridge, which has all the appearance of having been formed by folding. The morphology gives little support to the view held by Wanner and Brouwer in that the Strait of Manipa is a horizontal transverse fault. It is, of course possible, that horizontal movements occurred along the faults bounding the trough, but the ridge to the south has not been affected by these (compare the relations between the Jura and the Rhine valley-graben).

The troughs of the Halmahera Sea are known only from sundry wire soundings and one echo sounding section. The shape of the basins, and what is so far known of the cross-sections, are perhaps more in keeping with a faulted than with a folded nature of the trough. The positive anomaly of gravity supports this view. It can be that they should come under the second group of troughs.

In the West Indies there occur several kinds of troughs and basins. To Hess we owe a recent treatment of this subject (bibl. 52). Evidently the trough from Cuba, along Haiti, Puerto Rico and further along the Lesser Antilles, associated with a strip of strong negative anomalies, is of the same kind as our troughs of the fourth group. The Bartlett Trough is of a faulted nature with a positive anomaly, but the floor is more irregular, especially at the eastern end, than of our troughs of the second group. Hess offers a plausible explanation for the formation by tensional stress, but this explanation cannot be applied directly to the equivalent East Indian troughs, sothat there appears to be a slight difference between the two types. The Gulf of Mexico and the Caribbean Sea appear to conform very closely to our basins of the first group. No parallels are met with in the East Indies to the troughs occurring in the Bahamas.

D. Regional distribution of the scarps and other irregularities of the sections. (Plate IX).

The scarps.

Two aspects of the problem of submarine faults have already been noted. In the first place, the steeps which are to be found in the sounding sections may be much more precipitous actually than follows from the construction, and the structure may also be more abrupt than the actual rounded off forms. In the second place the number of scarps on the sea bottom must be very much larger than that marked on our map, Plate IX.

Our conclusions can only be therefore of a tentative nature.

Bearing in mind these limitations our scant data appear to accord quite satisfactorily with the deductions arrived at in the foregoing section.

On first sight of Plate IX we might conclude that the scarps and steeps are scattered at random over the entire region and that they show no systematic relation to the various types of submarine topography. However, we must not merely take into account the number of points at which the bottom steepens considerably, for a short distance, but also the declivity and the amount of the downthrow. If we leave out of account all the steeps which are under 20°, also those between 20° and 25°, of which the downthrow is less than 1000 meters, the remaining — more important — steeps and the vertical scarps are found to show a very significant distribution. We could, of course, also take another value, as forming the boundary between more and less important steeps. The reason that these particular numbers were selected is, that nearly all the steeps in the Java- and Timor Troughs are thus excluded. In this way the relative unimportance of the irregularities in these elements, is demonstrated more clearly than is the case with any other boundary.

Thus restricted, the distribution of submarine faults (?) is found to be as follows [1]):

		vertical scarps	important steeps	sum
1st group of neg. forms	Celebes Sea	1	5	6
	Sulu Sea	2	2	4
	N.W. Banda Sea	3	3	6
	S.E. Banda Sea	5	4	9
	(N. of New Guinea)	1	0	1
2nd group of neg. forms	Makassar Strait	1	3	4
	Gulf of Bone	0	0	0
	Gulf of Tomini	0	0	0
	(Halmahera Sea)	0	0	0
3rd group of neg. forms	Weber Deep	11	5	16
	Sawoe Sea	0	1	1
4th group of neg. forms	Java Trough	0	0	0
	Timor Trough	1	0	1
	(Aroe)-Ceram Trough	0	0	0
	(Boeroe Trough)	0	5	5
	Molukken Sea troughs	1	1	2
	Mindanao Trough	0	5	5
	(Flores Sea)	3	7	10
5th group of neg. forms	Manipa Strait	0	1	1
	Salajar Trough	0	3	3
posit. forms	Luymes-Siboga Ridges	1	3	4
	Toekangbesi Group	7	0	7
	Centre of Molukken Sea	0	3	3

From this table we remark: a) that all members of the first group of negative forms show a considerable number of submarine fault scarps.

The basin north of western New Guinea was only traversed once, so that the relative number

[1]) As the boundaries between the morphological elements are vague, there are a few cases in which it is doubtful to which element the scarp should be counted.

is not as low as it appears. This result conforms satisfactorily with our hypothesis, that these depressions were formed by primary vertical movements (block faulting).

b) that the members of the second group are not uniform. The Makassar Strait contains a fair number, the others (out of which two are known from one section only) show no scarps. The morphology of these is also less typical. The lesser degree of their (faulted?) subsidence may explain this apparent scarcity of scarps.

c) that the third group, notably the Weber Deep, shows a large number of scarps, which is in accordance with the box-like section.

d) that in the fourth group scarps are usually absent, or extremely rare. The Boeroe Trough, Batjan Basin and Flores Sea alone are more generally characterized by this form but it was also found, that in other respects they form a transition to the first or second group. Several of the scarps in the Mindanao Trough appear to belong to the ridges on the outer-, oceanic side, and therefore do not detract from the folded nature of this trough.

e) The Manipa Strait and Salajar Trough both show scarps, that accord with our former conclusion that they were formed in connection with faulting.

The irregularities.

On plate IX the outstanding irregularities in the echo sounding sections have been plotted. The distinction between four groups of various degrees of irregularity cannot be other than subjective. A moderately diversified portion of a section which is otherwise exceptionally plain, will appear more striking than when intercallated between portions having abrupt and pronounced changes in declivity. Furthermore with rough weather or for other causes the soundings may show too slight or too great variations. Still, I considered it as being worth while to incorporate the results of this investigation with that of the scarps on plate IX.

The distribution of the irregularities is not very systematic. In how far this is attributable to the above mentioned causes is, of course, difficult to say. The following comments may be offered.

The undermentioned basins are undiversified in the centre and are slightly irregular towards the rims, viz: Makassar Strait, Celebes Sea, Sulu Sea, S. E. Banda Basin, Weber Deep, Sawoe Sea, Timor-Aroe Troughs, Gulf of Bone, southern Halmahera Basin. Probably also: Gulf of Tomini, basin north of New Guinea. The Flores Trough and N.W. Banda Basin are already less smooth.

The following troughs are uneven, frequently more especially in the centre: Java Trough, E. part of Ceram Trough.

The following regions show considerable irregularities, viz: Luymes- and Siboga Ridges, Molukken Sea, Mindanao Trough and adjacent portion of the ocean bottom, Boeroe Basin and W. Ceram Trough.

The following parts are extremely irregular, viz: E. of Cape Mangkalihat, between Maratoea and Sibutu, Sangihe-Talaud region, Snellius Ridge, between Halmahera and Waigeo, Lifamatola Ridge, Sanana-Boeroe Ridge, Toekangbesi Group, E. of Kisar, W. of Jamdena, Banda Plateau and Inner Banda Arc, Salajar Trough and ridges from Salajar to Flores.

Viewed broadly the ridges are distinctly less even than the trough floors. In fig. 11 it is explained that the greater the depth the smoother the sounding section would be. Although this property of echo soundings may be partially responsible for the conclusion arrived at, we note as well the existence of irregularities at a great depth, and of shallow bottoms which are smooth. I feel that we are justified in assuming that, on the whole the ridges are actually more uneven than the troughs. This goes to indicate that folding and faulting are of greater importance in the structure of the positive forms.

2. AGE OF THE PRESENT RELIEF.

There are some indications concerning the age and rate of development of the present, strong relief of the East Indies.

It is known, that diastrophism was active in the East Indies in the Mesozoic era and that the principal orogenetic period fell in the Miocene (see Umbgrove, bibl. 119). Molengraaff (bibl. 83) believes that the formation of the troughs must have been the counterpart of the production of the geanticlines and that the latter were formed after the Miocene diastrophism.

This opinion can be accepted in respect of the troughs of the second type, but for those of the first type, the connection with the geanticlinal development is less certain. Still, the former greater extension of the land, deduced from the facies of the tertiary sediments on the surrounding islands, as advocated by Born (bibl. 16) and Umbgrove (bibl. 119), applies to both types. The formation of the geanticlines for the major part was attained after the mid-tertiary diastrophism, as follows from the oblique truncation of a number of the anticlines and from the raised plio-plistocene reef terraces. This last evidence in particular favours the development being still in full swing.

Support to this conception of a recent formation of the relief is given by the fact that the strong divergencies from isostatic equilibrium show a close relationship to the distribution of earthquakes (Vening Meinesz, bibl. 120), and the present relief (Kuenen, bibl. 68).

The general conclusion, therefore, is that the deep-sea troughs and geanticlines date from the end of the Tertiary and are still in course of further development. The rate of subsidence would have to be of the order of 1 mm per year. This does not appear to be an improbable amount.

Brouwer (bibl. 17, etc.) believes the formation of the troughs (of the second type more especially) was attained by gradual migration of the geanticlines (in the pre-existing depressions ?), whilst Stille (bibl. 109) attributes them to a broad undulation of the earth's crust since the orogenetic period of the Miocene. Reasons will be given elsewhere (p. 85 etc.) for following Stille in this respect. If, however, Brouwer's opinion is correct then the present forms are young, but a different and also strong relief existed already during the tertiary diastrophic period and possibly also before.

As neither the rate of sedimentation, nor the thickness of the deposits in the troughs is known, direct evidence of the age is not available.

Molengraaff drew the attention of geologists to the remarkable discovery by the Siboga Expedition in the Ceram Sea. At a depth of more than 1000 meters the dredge encountered a large field strewn with recent reef corals, coated with oxyde of manganese (bibl. 85, p. 332). As the nearest point at which reef corals can exist now is 40 km distant, Molengraaff suggests that a subsidence in the Ceram Trough of over 1000 m must have occurred recently. Although running fairly close to this point the echo soundings of the Snellius Expedition did not discover a marked ridge from which the corals could have rolled down. If sufficiently steep, there is just enough space remaining to place a slightly submerged bank beside the course of the dredging operation, in between the sounding sections.

There is one characteristic attaching to the discovered corals, however, which casts doubt on Molengraaff's tentative explanation. As they were caught in the dredge, and as they were coated with manganese, they had not been covered with sediments, although there can be no doubt, that particles are sinking to the bottom here, just as in all other parts in the same trough. Manifestly, a fairly powerful current continuously sweeps the bottom clean, preventing all finer materials from accumulating. This same current could also be held accountable for the transportation of the corals, by rolling them gently down a slope. As already stated, the existence of a slope cannot be excluded on the available soundings. The corals may, consequently, have grown on a site, which is much shallower than their present position, and no conclusion as to amount and rate of subsidence of the bottom can be drawn from them.

3. THE EAST INDIAN DEEP-SEA BASINS IN RELATION TO FOSSIL SEDIMENTATION BASINS [1]).

Many authorities have attempted to solve the problem of whether deep-sea troughs are geosynclines. A generally accepted opinion has not been arrived at. There are several reasons governing this. In the first place there exists lack of knowledge, both as to the properties of geosynclines and of deep-sea troughs. In the second place it has gradually transpired that geosynclines are of very different characters and if the results arrived at in a former paragraph, are more or less correct, the

[1]) A lecture on the subject matter of this section was delivered to the Geologisch Mijnbouwkundig Genootschap voor Nederland en Koloniën in November 1933. Several points were debated upon during the subsequent discussion. I have great pleasure in acknowledging this helpful criticism, which has led me to modify my views in several respects.

same can be said of the negative forms of the relief. Finally much depends on our definition of the word „geosyncline".

It would burden our task too heavily if we were to attempt an exhaustive study of this very complicated problem. Anyone, more conversant with stratigraphical geology, also having a wide experience of different types of geosynclines, would be more fitted for this ambitious undertaking. It is our province, however, to bring forward those facts that relate to the negative forms of the relief and appear to bear on the subject of geosynclines.

Let us first briefly consider geosynclines. Our problem, as stated, is rendered more complicated on account of the fact that there is no unity of opinion, as to the true definition of a geosyncline.

We can define a geosyncline as being any region in which the thickness of the sediments is great. On the basis of this definition all the East Indian basins must be or become geosynclines, for sedimentation is known to be active throughout and there is sufficient room, even without further subsidence, for the accumulation of considerable sedimentary deposits.

If the term „geosyncline" be restricted to regions in which the sediments for a considerable part of the stratigraphical column are also substantially thicker than in the surrounding areas we arrive immediately at a dead end, since we know practically nothing of the rate of sedimentation in the present basins and shelf regions. Moreover, as practically all submarine areas must show thicker deposits than the surrounding land areas, where denudation prevails, all parts of the seas might be geosynclines under this definition.

Some geologists have restricted the term to sedimentation basins, which have later been folded into mountain chains. But if we take the folding as a necessary part of the definition of the term „geosyncline" we exclude the possibility of finding a recent representative, for the fact that the folding of these yet has to take place.

Thus, it is manifest, that so soon as we essay a definition of the term „geosyncline", further debate on our problem becomes superfluous. Still we have a quite definite problem to investigate into, namely: whether basins existed anywhere at all in former geological periods, similar to those now to be met with in the East Indies, and if so what is the geological history of those basins?

If we are able to find such basins as these, we might, on the one hand, draw conclusions, as to the past and future possibilities of the East Indian basins; and on the other hand, the characteristics of — and the conditions within and surrounding — these basins, could aid in shedding light on the properties and conditions of former basins. Only if a parallel between the fossil- and the recent basins can be drawn, is it legitimate to utilise the knowledge of the latter in elucidating the problems of the former. This line of inquiry is evidently too important to be blocked by the initial difficulty of defining to satisfaction the term „geosyncline", for it is after all amongst the basins which have been denoted as geosynclines, that we must seek to find fossil equivalents of the modern basins of the East Indies.

To begin with, we could imagine five different histories for a deep basin:

1. Not raised.
2. Raised without folding, before the accumulation of thick sediments.
3. Raised and folded, before the accumulation of thick sediments.
4. Raised without folding, after the accumulation of thick sediments.
5. (Raised and) folded, after the accumulation of thick sediments.

If a basin of confined area is not raised, it will ultimately become filled up to, or above, sea level. Fossil representatives of this case could only be elucidated by borings. Knowledge of such basins must necessarily be restricted. Whether they will ultimately become folded, in cases where this has not already taken place, must remain unknown. Many blockfaulted graben appear to belong to this class and also the „Innensenken" of the alpine orogenetic systems. Such basins may also be represented therefore in the East Indies. As far as I am aware, however, such broad basins have been filled, gradually, during the subsidence, never showing a deep morphological depression. This does not rule out the possibility that some of our basins are of the same type, but that they have not been filled up with sufficient speed to counteract the subsidence.

Regarding the second and third possibility no representatives appear to have become known. Of the fourth case representatives are known, but they have not anything like the great thickness of sediment as those of the fifth group. The Colorado Plateau might be cited as an incidence. Of this

type of basin we can also expect to find recent equivalents in the East Indies, although the fossil basins are again found to have been characterized by sufficiently abundant sedimentation to compensate the sinking during their whole history.

The fifth possibility is by far the most important group. Hall first pointed out, that folded mountain chains have arisen from basins in which the sediments are much thicker than the deposits of the same age in the surrounding areas. A large number of such basins have existed therefore through all geological ages. For this type, Dana later suggested the term „geosyncline". As this kind of basin was so common in former periods, it might be expected to be found also amongst the present deep depressions of the East Indian Archipelago.

Having regard to its importance, we will consider in greater detail the type of basins from which folded mountain chains have later evolved.

In the majority of cases these basins appear to have presented the following features:

Oblong basins, filled with a thick series of sediments, rarely deposited in greater depths than a few hundred meters, and comparatively seldom formed of continental deposits. From these properties it follows that the basins were formed as subsiding areas on a continent that sank gradually deeper, whilst the sedimentation kept pace by continually filling them up to near sea level. Although the floor forms a trough, frequently several thousand meters deep, the basin never appeared as a deep depression in the face of the earth. *The most frequent type of sedimentation basin from which a folded mountain chain has been formed may therefore be represented among the shelf seas of the East Indies, but not among the deep troughs.* Thus van Es (bibl. 37) and Umbgrove (bibl. 118) regard the Madoera Strait as a recent geosyncline.

Other parts of the Soenda- and Sahul Shelves may be actively subsiding as well, but at so slow a rate that sedimentation continually succeeds in filling up the depression.

The important question now arises, whether no sedimentation basin, which was later folded into a mountain system, possessed during some part of its history a true oceanic depth, for it is only in that case, that we can hope to find a fossil parallel to the East Indian depressions.

Among others, Grabau (bibl. 43) and Leuchs (bibl. 74) believe that the depths never exceed some few thousand meters and that they are almost invariably limited to a few hundred meters. I believe it has been convincingly proved, notably by Molengraaff (bibl. 84) and Steinmann (bibl. 106), that this is not true, but that some, if only a few, sedimentation basins attain great depths in which true deep-sea sediments are deposited. Supposing Grabau were right, the deeper East Indian basins could be excluded at once, in our search for modern geosynclines, but it appears probable that some, now folded, basins were temporarily of great depths. Besides the lithological evidence in favour of the great depths there is an important consideration strongly favouring the possibility of some geosynclines becoming very deep.

Morley Davis (bibl. 86), Lawson (bibl. 72), Chamberlin (bibl. 31), Grabau (bibl. 43, p. 345—346) Umbgrove (bibl. 118), Cornelius (bibl. 32), Stille (bibl. 109, p. 6) have shown, that the slow subsidence of the bottom of a geosyncline cannot be explained by the weight of the sediments alone. Hall, the originator of the conception of geosynclines, believed that this was the case. Daly is more or less of the same opinion (bibl. 34, p. 203—206) and Pruvost too (bibl. 88) inclines to attribute the actual sinking to the weight of the sediments. It was pointed out by the authors named, that the weight of the sediments when isostatically compensated by subsidence could only result in a sinking equal to about $^2/_3$ of the sediments own thickness. It was shown also that the subsidence is limited to a part of the area on which sedimentation commences.

We cannot deny, that the general rule, that most sedimentation basins were maintained at sea level, is difficult to explain. On the other hand, the theory of the weighting by sediments is not of much assistance, for this principle would act precisely in the same way for a basin that was deep to begin with and maintain the great depth; and if a deep basin was gradually filled up to sea level, there is no reason why the process should not continue, although gradually decreasing, and sedimentation remain in excess of subsidence. The weighting theory furnishes no explanation of why the constant level maintained is usually close to sea level.

In my opinion this problem may find its solution, partly at least, in a different direction. A deep basin will trap all sediment that is brought into it by external forces and organisms. The

shallower it becomes, however, the more sediment will be lost, by currents and waves that carry the finer debris away to deeper and calmer waters. In the case the sediments come to lie above sea level, the balance will be swung even further in favour of transportation of debris to other parts. The principle is the same as that invoked by Davis (bibl. 35, p. 16, 97, 135 and 545) in explanation of the constant and limited depths of lagoons of coral reefs. According to this principle, a basin will be maintained at moderate depths for as long as sedimentation is slightly in excess of subsidence. There is nothing extraordinary about the fact that this balance was often obtained [1]). Furtheron I shall point out why the balance was not maintained nearly as accurately in some cases as is frequently believed.

In any case it would seem certain, that the load of the sediments may be an important factor in augmenting and quickening the subsidence, but that there must be some other, primary, cause for the sinking. The filling in of this depression is therefore a secondary phenomenon resulting from external causes, namely: erosion and transportation. Steinmann (bibl. 106) and Stille (bibl. 107) have already drawn attention to this conclusion. Even, if, as Grabau believes, the rising of the crust adjacent to the basin is a phenomenon always accompanying the formation of the depression (and this is by no means established beyond any doubt) even then, it is a matter of coincidence whether the depression will be kept almost full during the whole of its history. The relative size of geanticline and geosyncline together with their level with regard to sea level, the rate of their formation, the nature of the rocks exposed, the climate and oceanography of the sea and the orientation of the drainage, constitute so many factors controlling the rate of sedimentation. Although the study of sedimentation basins has shown that these factors often tend to counterbalance one another and to keep the depression permanently filled up to a level only slightly below the surface of the sea, it is certain that they will not always do so. In my opinion we cannot dispute that in some cases a subsiding basin was not sufficiently supplied with sediments and increased in depth gradually. We are at a loss to say to what depth an empty trough could sink, for that would depend on the extent of the primary causes responsible for carrying the bottom downwards and of these causes we know nothing for certain. There is no reason for doubting that depths comparable with those of modern deep-sea troughs could be attained.

The next point to be considered is, whether a great depth is characteristic of a particular type of geosyncline, or whether it might occur in any type of geosyncline.

Schuchert (bibl. 97) believes, that there are two leading types of geosynclines. The one embracing the mono- and poly-geosynclines is characterized by shallow water conditions throughout and is initiated on a continental block; the other, meso-geosynclines, in which abyssal sediments occur amongst shallower types of deposit, is situated between two continental masses. Some doubt arises, concerning whether this distinction is correct.

In the first place the considerations presented above would lead one to assume, that any geosyncline could become deep providing the supply of sediment were to decrease and that this supply does not depend upon the geosyncline itself and cannot, therefore, be regarded, as characterizing the type, from a genetic point of view.

It is indeed true, that a depression forming on a continent will receive a larger amount of sediments than one situated beside a continent or between two fairly distant masses. This would serve to explain why in this case the depth usually remained small; but it appears doubtful whether such a rule is sufficiently strict to be claimed as distinguishing a separate class.

In the second place the proto-type of the second kind of geosyncline given by Schuchert is the Alpine geosyncline, but it is now known that this geosyncline was formed originally on a continent, only attaining its great depths in later stages of development. Thus, Heim states (bibl. 49, p. 49): „Die Erscheinung einer transgressiven Auflagerung der Trias auf dem Altkristallin . . . beherrscht die ganzen Alpen". p. 499 „Prätriasisches Alter der kristallinen Deckenmassivgesteine, Denudation gegen Schlusz der paläozoischen Zeit, Küstenerosion und Transgression des Mesozoikums". The great depth was attained during the Jurassic (see also Cornelius, bibl. 32).

In the third place it is possible, that the sediments in many geosynclines were deposited in much greater depths than the facies would lead us to suppose. This suggestion may sound bold, but I have two reasons to submit in favour of why it could be true. Escher has pointed out that submarine

[1]) The writer finds that nearly 20 years ago Barrell proposed the same theory in much greater detail and in more convincing form (Bull. Geol. Soc. Am., 28, 1917, pag. 776—785).

landslides may have played an important part in filling deep-sea troughs with sediments (bibl. 38). These would tend to bring shallow water deposits into considerable depths. This problem is treated in another part of the present volume.

The second reason is based on experience gained during the Snellius Expedition. Below a depth of a few hundred meters the depth by itself has no direct influence on the deposit collecting on the bottom. It is solely the distance from the coast and the chemical and physical properties of the water in the basin which influence the sediments. The closer in-shore, the coarser and the more of inorganic matter will sediment and, consequently, the steeper the trough wall, the greater the depths at wich a given type of sediment will accumulate. Further, the deeper the basin, the greater is the amount of carbonate of lime that is dissolved by the bottom water. But this is not true for basins with a very shallow communication with the ocean. In such cases the circulation of the water is insufficient for refreshing the bottom water rapidly enough to allow of dissolving all the calcareous sediment that sinks to the bottom. This may be exemplified by comparing the Celebes Sea with the Sulu Sea. Both are about 5000 m deep, but the former communicates with the ocean over a sill of about 1500 m in depth, whilst the latter is cut off from the open sea below the depth of some 300 m. The outcome of this is, that the sediment forming on the bottom of the Celebes Sea is entirely free from carbonate of lime, whereas the bottom of the Sulu Sea is covered with globigerina ooze composed almost exclusively of carbonate of lime. Similar globigerina ooze does not occur in the Celebes Sea below a depth of some 3000 m. From the nature of the ooze forming in the Sulu Sea alone we could not determine whether it had been formed at a depth of a few hundred meters, or of several thousands of meters.

Thus, we find that there are, at least two reasons why, in some cases, it is not possible to conclude whether a given sediment was finally deposited in depths of a few hundred or of a few thousand meters. Conversely we must place a questionmark to the conclusions arrived at, pertaining to the depths of geosynclines when we are not dealing with actual shallow water deposits, or when other conclusive indications of depth are absent.

The fourth reason for doubting whether Schuchert's classification of geosynclines is based on valid principles for a genetic distinction, is encountered, when we consider the Timor- and Java Troughs. It is repeatedly pointed out in other parts of this volume, that these two structural elements form parts of the selfsame major feature of the earth's crust. The one is the direct morphological continuation of the other, both are situated on the convex side of the same geanticlinal ridge, both represent a synclinal form, that is 1—3000 m deeper than the foreland and 2—4000 m deeper than the geanticline which they follow, they are of the same size and general ground plan and finally, they both follow along the outer side of the belt of strong negative anomalies of the gravity.

There is no gainsaying the fact, that the Java Trough and the Timor Trough belong together and form one single trough. The Java Trough is not a geosyncline in any of the senses defined by Schuchert, for it is neither situated between two continents, nor is it a shallow basin formed on one continent. Yet if the Timor Trough is a geosyncline, then the Java Trough must be one as well. Even if we consider Schuchert's para-geosynclines, which are formed on the edge of one single continent then the Timor-Java Trough is a representative of two classes as regards different parts of its length. In short the position of a geosyncline with respect to continents is not a fitting criterion for distinguishing types.

The same example also illustrates another reason for belief that depth is neither of classifying importance. The depth of the Java-Timor Trough varies between 7000 and 1500 m. If one single geosyncline varies in depth so enormously, it serves to indicate that this element is too variable to form a sound basis for genetic classification. Of course, it has still to be shown that the Timor Trough is a recent geosyncline. I will attempt to offer proof of this further on.

In summing up the tentative results, we find that neither the position of a geosyncline with relation to continents, nor the depths are quite satisfying properties for a genetic classification. Possibly a more satisfactory result could be obtained by taking into account the size of the geosyncline, the degree of subdivision by secondary geanticlines, the length of its duration, the amount of latteral displacement — that is of migration — the breadth compared with the length, the degree of tectonic activity during and after the sedimentation. Some reasons will be given for believing that the cross-section also might be a sound element in classifying geosynclines.

It is further of great importance to know whether the sedimentation basins, that were later folded into a mountain chain, were produced by faulting or by warping of the earth's crust. Stille (bibl. 107, 109) has devoted especial attention to this question. He came to the conclusion, that the rather generally accepted opinion, that faulting is the principal mechanical deformation that leads to a geosynclinal depression lacks all soundly established grounds. Although at present faults frequently bound the geosynclines, on closer examination they appear to have been formed during a later stage and not during the subsidence of the trough. On the whole, we find that the thickness of geosynclinal sediments vary in the cross-section gradually, and the same can be said of the facies of the rocks (see p. 83 for further conclusions of Stille on deep-sea troughs).

It has been shown, however, that in many cases the geosyncline migrated gradually during the development of the mountain system which has sprung up from it. Although this displacement is more or less abrupt, there is nothing in the nature of the movement in favour of the faulting hypotheses. See among others Grabau (bibl. 43), Cadish (bibl. 30).

We cannot deny the possibility, that some few basins, later to be folded, may have sunk down along great faults, or step-faults; but they would be exceptions. In our quest for modern equivalents we may assume, that the troughs having a faulted nature are not representatives of the *normal* type.

A final characteristic of many basins is, that they lie on the convex side of a rising geanticline, which discharges its denudation products into the trough. It is a matter of doubt whether this feature is invariably present, but the inorganic element of the sediments must have come from somewhere and for a geosyncline which has already gone through the first orogenetic period, it appears to be typical. Thus, the presence of a rising geanticline, on the concave side of a trough, would be a strong indication that the basin was of the same nature as those which later formed orogenetic systems. Still the absence of a rising geanticline does not furnish proof of a different nature of the depression.

I am well aware, that these remarks upon „geosynclines" are sketchy; but as I have pointed out above, this aspect of the problem cannot be gone into „in extenso" here.

Having briefly reviewed the principal characteristics of various kinds of fossil basins we are now in the position to make our search for modern representatives in the East Indies. Molengraaff (bibl. 85, p. 292), believes that all the deep basins of the East Indies are geosynclines. We shall have to consider the groups of troughs that we can now distinguish, separately, since they need not all fall under the same class.

We will commence with the Timor Trough as possessing the features that appear to be most readily identified.

Grabau's definition and description of a geosyncline fits the Timor Trough in every detail that we can ascertain at present:

„The geosyncline is a structural feature of a continental block of the earth's crust. It is a long and narrow subsiding area parallel to an old land which supplies much of the sediment. The old land is an important supplementary feature of the geosyncline and is always present, forming indeed its counterpart, for while the geosyncline is a subsiding area the old land is a steadily rising area. In no other way can the uniformity of supply of sediments be accounted for." „The deepest portion of the geosyncline is narrow and from this there is a steady and more or less gentle rise towards the continental platform or marginal plain which borders it on the side opposite to that on which lies the old land" (bibl. 43, p. 214).

Timor represents the „old land" and it is actually steadily rising, whilst the Sahul Shelf represents „the continental platform." The comparison goes even further for Timor is a folded geosyncline and the geosyncline would therefore have migrated in the normal manner and now lies, if the whole arc is considered, on the convex side of the older mountains. Further, the Timor Trough lies just outside of the belt of great negative anomalies of the gravity, thus in a region, which appears to be destined to undergo still further tectonic activity. The section of the trough complies with the definition and the recent sediments are formed of fine continental waste containing a varying proportion of carbonate of calcium from planktonic sources. All the characteristics of a geosyncline having considerable depths are thus found in the Timor Trough and no features contrary to this interpretation appear to exist. So far as our present knowledge goes, we are justified in regarding the Timor Trough as a modern geosyncline in act of formation. It is not comparable to the tertiary,

Molasse-geosyncline of the Alps. To what stage and part of the remainder of the depressions of the alpine system it most closely approximates is difficult to say. It might represent the northernmost trough of the Penninic geosyncline, or the Helvetic trough after the depression which it went through before being finally overwhelmed by the forward thrusting nappes. This problem, as has been stated, can be placed safely in the hands of specialists in alpine geology.

It was shown above, that if the Timor Trough is a geosyncline then the Java Trough must also be one; and the continuation — in the opposite direction — into the Aroe Basin and the Ceram Trough, is a no less typical representative of a modern geosyncline. As the troughs at both sides of the Molukken Sea and the Mindanao Trough bear the same relation to the belt of negative anomalies and present the same cross-section and general ground plan as the Java-Timor-Ceram Trough they, too, fall under the class of modern geosynclines. In other words, all the representatives of our fourth group of negative forms are recent geosynclines. Haug was the first one to regard the Java Trough and the circum-Pacific troughs as modern geosynclines (bibl. 48, and his Traité de Géologie, p. 157—167). In this respect, therefore, the new data still bear out Haug's opinion formulated a quarter of a century ago.

In the same way as the position of the Flores Sea in our morphological classification was doubtful so too must its relation be to geosynclines.

Although a prediction of the future history of these troughs is a bold undertaking and — by the nature of things — controversial, there is a sufficiency of fact upon which to found an opinion. The large number of cases in which similar troughs have become charged with sediments and have been subsequently intensively compressed to folded mountains, frequently with overthrusts and nappes, is a strong indication to the effect that the troughs of our fourth group will eventually go through the same process of development. Whether they will migrate further outwards is more doubtful. Possibly, the depression of the shelf opposite to Jamdena, in the Tanimbar Group, constitutes the first indication of future development in this respect.

In turning to our other groups of troughs, we are faced with greater difficulties.

The first problem is that of the cross-sections. For all these basins the section is box-like with a flat bottom and relatively steep sides. If we are right in our belief that the sides are of a faulted nature, or formed by flexures, and if, on the other hand, Stille's opinion is correct viz: that the basins which gave rise to folded belts were not bordered by faults, then these basins are of a different nature. Grabau's conclusion, that the floor of the fossil troughs rose gradually on the outer side towards the foreland would lead to the same opinion.

Yet again one must admit that it would be next to impossible to prove in most cases that in no stage of the development of the fossil basins was the bottom horizontal. We must not lose from sight the fact, that the slopes of our basins are only relatively steep, but actually very gradual. Additionally the alteration of the sediments, from the border inwards, does not depend alone upon the slope and depth, but also upon the distance from the coast; and as the latter increases by degrees the character of the sediment may also change gradually irrespective of what the section is. On being examined in a folded chain, such deposits might not show any indication of the (relatively) abrupt nature of the slope into the trough in which they were formed.

The section can, therefore, convey only a slight indication to the effect that the basins of our first three groups may not belong to the class from which folded mountains are formed. But this trend of reasoning certainly shows that, in the future, important results may be obtained if stratigraphical geologists are able to ascertain the cross-sections of the basins in which the sediments were laid down. More attention must be given, in the future, to this side of stratigraphical geology.

The second problem is that of the ground plan. As regards the members of our first group of basins the great breadth, or even the round shape render it improbable that they could be folded into a mountain system. The basins, from out of which the latter were formed, were invariably comparatively narrow, compared with the length; notably the larger examples comparable in size to basins such as the Celebes Sea. There is more probability in the supposition that the basins of the first group are deeplying „Innensenken'', such as the Hungarian plain in the Alpine system, a possibility already suggested by Staub (bibl. 105). It still remains to be found, whether these structural elements ever attained great depths; but the possibility cannot as yet be ruled out. The little which is known of the structure of the areas surrounding the basins of the first group can scarcely be cited in support

of this explanation. Nothing has been found showing that these areas are comparable to the overthrust chains around the typical „Innensenken".

For the members of the second group of basins the ground plan is less unfavourable to a comparison with fossil depressions that were later folded, as they are more oblong. The sudden changes in breadth on the Makassar Strait and the abrupt ending at one extremity of all, their evident relations with the members of the first group remain adverse to this interpretation. For these basins the interpretation as fault troughs, graben, lies at hand.

The third group comes still nearer the required shape. The slower changes in breadth, the oblong shape and curved ground plan should be noted.

The third problem is that of the position. The members of the first and second group of basins are not linked in long festoons that resemble the orogenetic belts of the world. Neither are they so intimately linked with geanticlines of orogenetic origin as those of the other groups. Again, they are not related to the line of strongest negative anomalies, with earthquakes and volcanoes. The gulfs of Bone and of Tomini, it is true, are situated on the convex side of the (probably?) less important belt of negative anomalies, following the eastern arms of Celebes. But with this reservation only, the position of the first type of basins is thus less favourable than those of the second type, to an interpretation as future sites of diastrophism.

The third group of basins is situated more favourably in this respect. These form a continuous string, are close to the main belt of negative anomalies, the line of most earthquake epicentres, and the line of recent volcanoes; and, lastly, they are situated alongside two geanticlines in one of which, at least — and probably in both — orogenetic activity played an important part.

A final remark must be made upon the rate of sedimentation in the troughs. It might be supposed, that the very slow supply of sedimentary matter in the troughs is contradictory to the rapid accumulation of deposits in geosynclines. But it must not be forgotten that the time during which sediments accumulate, even in shallow-water geosynclines, is of tremendous length. The 20.000 m of deposits of the Palaeozoic Appalachian geosyncline were deposited over more than 200.000.000 years or at a rate of more than 10 years per mm. The jurassic strata in the helvetic trough of the east of Switzerland are only a few hundred to one thousand meters in thickness, although that period covered some 30—40 million years. In this case it took from 30—100 years to deposit but one millimeter of sediment, in comparatively shallow water.

Unfortunately we know next to nothing of the rate of sedimentation in the East Indian basins, but there is no reason for supposing that it is slower than the rate at which sediments accumulated in other deep geosynclines.

Summarising this section we come to the following conclusions:

The troughs of our fourth group are found in many respects to resemble the sedimentation basins from which mountain chains, comparable with the Alps, were afterwards formed. It is not with the alpine geosyncline as a whole, but rather with its secondary depressions, that we encounter such a striking coïncidence.

The troughs of the third group probably also represent depressions in a complex, comparable with the alpine geosyncline. The only respects in which this interpretation is less certain is: that the breadth varies more, that the section is more abrupt and box-like and, finally, that the position is on the concave side of the outer arc with nappe structures, while so far such structures have not been proved for the inner arc, with the (doubtful) exception of Java.

The troughs of the first type are different. The following features were encountered rendering a comparison with the sedimentation troughs of folded chains doubtful. The abrupt box-like section, the abrupt variations in breadth and the abrupt termination, the absence of long continued festoons, the absence of any clear and close relation to an orogenetic arc having a strong negative anomaly of the gravity, instead a strong local positive anomaly. The interpretation as „Innensenken" is doubtful. The basins of the second group might constitute fault troughs.

It should be again emphasized, that the present writer has no wish to turn the matter round and consider the troughs of the second type as typical geosynclines. It is sufficiently firmly established that the majority of fossil sedimentation basins have never attained more than a few hundred meters in depth.

4. TREATING OF THE MEANING OF TECTONIC LINES ON A STRUCTURAL MAP OF THE EAST INDIES.

Several geologists have drawn tectonic lines on the map of the East Indies, or else have proffered opinions upon the connections which are believed to exist (see the next chapter). In some cases, however, it is not quite clear what is meant by these connections. Between well-founded conceptions, on the one hand, and entirely hypothetical decorations of the map, on the other, all manner of more or less vague notions have been expressed.

A structural line might thus be taken simply to mean a morphological connection. The new chart already shows comprehensively what manner of morphological connections exist (see also fig. 47). But a morphological connection is not necessarily a tectonic connection as well, and cannot be made use of in a tectonic map without careful reflection.

A structural line might also be drawn through the centre of a tectonic zone. In such a case, it might run obliquely to some, or all, of the morphological or tectonic forms, and would denote the general direction of the tectonically active belt.

The line could be the central line of a geanticline also, or only that of an anticline, as also of a geosyncline or a simple syncline.

Finally, a block-horst could be denoted which might run obliquely to either the tectonic zone or to the direction of the thrusting.

In some cases stratigraphical data have been taken into account in the drafting of the connections. Stratigraphical relations are the outcome of the relief at the time of sedimentation and thus of the then-past tectonic and, or, volcanic history. Although a stratigraphical unit will also frequently form a structural unit afterwards (for example a geosyncline) this need not always be the case. For this reason stratigraphical data can only be made use of in a subordinate place and in connection with other data, when our structural line is intended to denote present structural relations.

There is, however, still another reason why stratigraphical data must be used with discretion. In order for this to be understood we must offer:

Some remarks on the influence of relief on the stratigraphy.

The influence of the relief upon the character and the amount of the sediments deposited, is a problem that should in point of fact be considered in connection with the sediments themselves. It does not, therefore, belong to this volume which treats of the bathymetrical results of the Snellius Expedition. There are, however, a few questions respecting the relation existing between relief and sediment, which must be pointed out here, in order to gain a clearer conception of the meaning of tectonic lines on a structural map.

Generally speaking, a morphological unit will also form a stratigraphical unit, although gradual transitions of the strata, both in character and thickness, may occur. Thus, the lime-free, fine deposits forming in the Celebes Sea are similar for the entire deep part of this basin. Towards the margins it shows everywhere the same increase in carbonate of lime, this element decreasing again relatively towards the coasts, where more and coarser continental waste matter is added. In the northwest, the east and the southeast, volcanic matter plays an important part.

This general rule is not to be taken as a fixed law, however, as some morphological units may show great variations in their deposits. Thus, the trough along the Outer Banda Arc is generally characterized by fine sediments, with a considerable quantity of carbonate of lime, but in the deepest parts this element is dissolved. Morphologically, this trough is continued to the west, in the Java Trough; but, here, the relative amount of continental waste decreases to almost zero and true oceanic sediments prevail. The sediment forming on the bottom of the Mindanao Trough contains more continental waste in the deep part, opposite Mindanao, than further south, where it is bounded by only a few small islands; and still further along, the depth decreases, with consequent increase of carbonate of lime.

The positive forms of the relief are subject still more to variations in the sediment deposited on them. When parts emerge, they are eroded, whilst other, submerged parts of the same ridge are still subject to sedimentation. If the submerged parts form the sill between two basins, strong tidal currents will sweep the bottom. The erosive power of such currents is probably slight in most cases, but it effectually obstructs depositing. A fine example of the latter case is the connection

between the Soela Islands and Obi. This ridge, although nearly 2000 m deep in the central part, is denuded of sediment and is covered with a thin coating of manganese oxyde. The same is to be observed in most of the sills between the great basins of the East Indies, as Weber (bibl. 126) has already pointed out[1]).

On the other hand, a ridge which lies along the centre of a basin, will hardly differ in sediment from the surrounding bottom, provided the difference in depth is not too great.

From these considerations we learn, that in drawing structural lines we must be careful in utilising stratigraphical data. A region containing similar sediments may have been divided by submerged ridges at the time of sedimentation; even partially emerged ridges might subsequently escape observation, if the amount of exposure is small. On the other hand certain variations in sediment can occur in one and the same morphological unit.

The foregoing remarks will sufficiently show that a structural map is of value only, when a precise statement is added as to what the lines stand for.

Umbgrove points out several instances in which the structural maps drawn by several geologists are not only highly hypothetical, but in which use is made of different types of data for deliniating one and the same line (bibl. 119).

In the present author's opinion there is still too little known about the tectonics of the Molucca's for justifying an attempt at offering a new structural map. The tectonic map, including both stratigraphical and structural data recently published by Umbgrove (bibl. 119) summarises our present knowledge, in the best manner possible. A mental projection of this map on the bathymetrical chart of the Snellius Expedition, together with the gravimetrical data and the remarks offered in a foregoing section — on the most likely nature of the structure of the various positive and negative forms (summarised on fig. 47) — appears to be the closest approach that we can attain as yet towards an ideal synthesis of the structure of the Molukken region.

5. A COMPARISON BETWEEN THE EAST INDIES AND THE ALPS.

One of the most important problems of geology is to gain an insight into the complete history of a folded mountain chain from the firt stages of sedimentation, through the great geosynclinal phase and the subsequent orogenetic stages to the final denudation and isostatic compensation of the region, when the prolonged cycle has drawn to its temporary close. It is not possible to come to a full understanding and a complete knowledge of this great epic of an orogenetic belt, solely by the study of a region that has already completed the whole cycle. It is only through the minute examination of a number of such belts in different stages of development that we can hope finally to arrive at a complete and well founded conception of the orogenetic cycle. As, however, all better known mountain systems of the earth are somewhere in the final stage, where compression has practically ceased and where denudation and compensation have taken the upper hand, they do not form a complete set with representative cases of all stages of the orogenetic cycle. The investigation of regions in the geosynclinal or compressional stage of orogenetic development therefore assumes a greater and wider importance than merely to ascertain their present characteristics. Such regions, by demonstrating the first two acts of the great drama of orogenecy, can become of unique importance to the student of mountain formation.

The geosynclinal and compressional period are generally described as having followed eachother in the formation of a mountain chain. This is only true in a general sense and perhaps for simple mountain systems. A detailed study of the greater systems reveals that the compression sets in at an early stage and is repeated more or less spasmodically during a considerable, sometimes even the principal, part of the geosynclinal stage. More or less important geosynclinal formations are formed even during the principal orogenetic convulsion and continue to form long after the compressional forces have already arrived at an older stage. In the search for modern geosynclines and orogenetic belts which are undergoing compression we must sometimes expect to find the two combined in one and the same belt.

---

[1]) See also Andree, bibl. 2, p. 16—18 and p. 199—200.

The present author is convinced that in the East Indies we have a wonderful instance of a combined geosynclinal and active orogenetic belt. This idea is by no means original, for many investigators have already come to the same conclusion. It is well, however, to draw attention to this conception and to add some new arguments in its favour. If this theory is correct, the study of the East Indies should not only be undertaken on account of its own interesting features, but also on account of its importance as a stage in the orogenetic cycle which has hitherto been insufficiently examined.

Our problem is of a double nature. The examination of the region with a view to finding geosynclines has already been made in a foregoing section of this chapter. We will now consider whether the East Indies can be compared with the Alps and to which stage of the development of the Alps they are most closely related. Obviously, this problem could be tackled with more success by an Alpine geologist. The following remarks must therefore be taken as suggestions to be considered by one who is more qualified to throw light on the comparison from the opposite side.

Argand (bibl. 3) first drew a clear parallel between the Alps and the east part of the East Indies, comparing the latter with the former during the Jurassic, but without adding any details. In general terms Molengraaff stated his belief that the Molukken represent a mountain system in the act of formation (bibl. 82, 83, 85). Staub (bibl. 105) reemphasized Argand's statement to the effect that the ground plan of the East Indies is in many respects comparable to that of the Alps, but he omitted to consider the important problem of the relative stages of development. Brouwer, on the other hand, stressed this aspect of the problem. He showed that already in the Upper Triassic the Molukken showed geanticlines and geosynclines, as the rocks of this age on Timor show strong variations in facies. The Alps only attained this stage in the Jurassic, but subsequently showed a swifter development, so that they have now left the Molukken far behind.

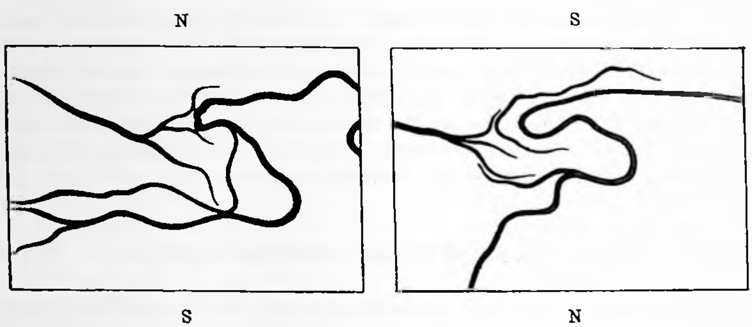


Fig. 26. Comparison between the structural plan of the alpine system of Europe and the East Indies by Staub, bibl. 105, fig. 24, p. 90.

The chief points in which the two regions appear to resemble eachother are the following:

1) As Argand and Staub pointed out, the groundplan is distinctly similar. The two regions are rotated 180° with respect to eachother, so that we must place north at the bottom for the East Indies in order to appreciate the resemblance (fig. 26). We then see that both consist of an arc with the strongest curvature on the left. From this point projects a separate system: the Pyrenees and the Snow Mountains of New Guinea. In some respects Staub's drawings must be altered. Thus the Banda Arc is not continued to the Palao Archipelago, but towards Mindanao. Some further, less important changes are required, but these need not be mentioned separately.

2) An addition must, on the other hand, be made. The peculiar position of the Italian peninsula with its Mediterranean volcanic rocks is recalled by the position of the strip of Mediterranean volcanic rocks along the west and south part of Celebes and on as far as Tana Djampea.

3; Argand showed that during the Jurassic the Alps presented a stage of development, in many respects similar to the present conditions in the Molukken. There were two island arcs with biogene reefs and deep basins between. We could add that Kisar might form a parallel to the Monte Rosa nappe.

4) As in the Alps, nappes have been formed in the East Indies, although it is not yet known to what extent.

While enumerating these points of similarity we must not overlook the considerable differences that doubtless also exist. The following may be mentioned:

1) The elements in the East Indies are considerably larger than in the Alps.

2) Large parts of the East Indies are surrounded by oceans, whereas the Alps s.s. appear to have been surrounded by continents on all sides.

3) The volcanic activity in the East Indies is much greater than it ever was in the Alps, except perhaps during the Permian. In that period, however, the Alpine region had not yet attained the general appearance of an arcuate archipelago with deep basins.

4) Brouwer drew attention to the existence of coarse, clastic sediments and deep-sea deposits in the East Indies already during the early Mesozoic. He pointed out that if the two regions are developing in the same direction, the Alps were carried through the cycle much swifter than the East Indies.

5) Along the crests of the Alpine nappes coarse conglomerates and breccias were formed during the development of the island arcs, especially along the convex side. It is doubtful whether such rocks are also being produced in the Molukken at the present time. My experience in these parts was that with only very few exceptions, soft or sandy deposits are being formed quite close to the shore. As, moreover, the Outer Banda Arc is steeper on the concave side, there seems to be more chance of the depositing of coarse sediments on that side. It is also doubtful whether radiolarian ooze is being formed anywhere in the archipelago in the same manner as in the mesozoic alpine and East Indian geosynclines. These matters will, however, have to be reconsidered in the report on the deep-sea deposits.

With regard to the other side of our comparison we have already learnt that the troughs along the island arcs, i.e. the Java Trough, the Timor-Aroe-Ceram Trough, the troughs of the Molukken Sea and the Mindanao Trough are modern geosynclines, similar to those of the Alps in the Mesozoic. Probably the Weber Deep, Sawoe Sea, and the basins along the south coast of Java and the south-west coast of Sumatra are also modern geosynclines. All the other basins are of a different nature and appear to have been formed by a primary vertical force, while it remains doubtful whether they are similar to the geosynclines from which orogenetic mountain systems have risen.

Taking these matters into account we come to the following conclusion concerning the comparison of the Alps with the East Indies:

The Alps and the East Indies are sufficiently alike to justify the opinion that in the latter region we are witnessing the earlier stages of a mountain-building cycle. The seismic activity and the abnormally large deviations from isostatic equilibrium, the strong progressive upheaval of many of the islands are all in accordance with this view. Consequently, geologists who are studying older orogenic belts may find the solution of many of their stratigraphical and even of some of their structural problems by a comparison with the East Indies.

Although the Alps and the East Indies evidently belong to the same class of orogenic belts, the differences in size and relative importance of the features, and of the rate and sequence of the stages of development are sufficiently large to show that the laws according to which they are formed are not hard and fast rules. There are not sufficient grounds for maintaining that the East Indies will ever develop into an Alpine chain of mountains whose structure is ruled throughout by nappes. On the other hand, the likelyhood that this will ultimately occur seems considerable.

6. RELATIONS BETWEEN GRAVITY FIELD AND MORPHOLOGY.

In a separate publication (bibl. 68) forming part of bibl. 120, the present author has considered the relations between the gravity field in the East Indian Archipelago and the morphological shapes. A summary of the principal results will be given here for the sake of completeness.

The basins of our first group show a varying but strong positive regional isostatic anomaly of about 80 milligals. Those of the second group are characterised by 50 milligals positive anomaly. The members of the third and fourth group are all situated along the belt of strong, negative anomalies, discovered by Vening Meinesz. The Aroe- and Boeroe Basins are situated at points where

the string of basins of the fourth group deviates from the line of negative anomalies. Where the Mindanao Trough deviates to the south-east from the line it decreases in depth and tails off to the east of Halmahera. The troughs on both sides of the Molukken Sea bend away from eachother in the south at the same place where the line of negative anomaly broadens and sends off a branch to the north-east arm of Celebes.

The Australian Continent influences the direction of the line of negative anomalies, but hardly its character and intensity. There is nothing in the gravity field to indicate either that the Australian block was forced up against the arcs, or that the Outer Banda Arc was originally a regular curve and was subsequently moulded up against the already existing shape of the continent.

All the ridges with interchanging crests are situated above or close to the line of strong, negative anomalies.

Thus there is found to exist a marked relation between the field of gravity and the relief. This correspondence is not rigid, however. The principal reason must be that the forces causing the deviations from isostatic equilibrium lie in the lower layers of the crust, whereas the relief is caused by the reaction of these forces on the upper layers.

7. SUBMARINE SLOPES OF VOLCANOES.

Before entering into the subject of the submarine slopes of volcanoes we should first attempt to analyse the subaerial slopes. Although this problem is of great importance to the morphology of volcanoes it appears to have received little attention from scientific investigators.

As early as 1878 Milne (bibl. 81) drew attention to the slightly concave slope of most regularly shaped volcanoes. He and Becker (bibl. 6, 7, 8) both believed that the principal cause of this concavity is that a volcanic pile compresses its lower strata by sheer weight, causing them to spread out laterally. Von Wolff (bibl. 130) pointed out that this can hardly be the case, as other structures, similar in shape, do not spread out by their own weight.

Judd and Woodward made experiments to ascertain the shape of cones formed by the piling up of ejectamenta round a vent (bibl. 60). They concluded that the slope is straight. Later Linck, without knowing of these experiments, made a similar experimental investigation (bibl. 76). He found that only the lower part is concave, the middle slopes being straight or slightly convex, while the higher part is markedly convex. It was a mistake, however, to apply the results to strato-volcanoes. The experiments were stopped as soon as material started to roll down the slopes, thus only illustrating the formation of a volcanic embryo with a crater wider than the cone is high. Owing principally to the presence of lava and agglomeration, the vent in a large volcano grows upwards, instead of remaining on the original level, as in the experiments. The experiments should have been carried on much longer, while continually raising the aperture of the vent in the volcanic pile. In that case the process of the rolling of the particles down the slopes would become of increasing importance, finally overwhelming all but the upper and lower parts of the slopes. Almost the entire cone would thus assume the natural slope of the materials used.

In an actual volcano there are, however, still further influences to be considered. The materials vary greatly in size and shape and each has its own natural slope. A slightly concave slope might result, as the material with the smaller natural slope will run further down the flank of the volcano. Milne already indicated this result.

In most strato volcanoes the amount of lava is not sufficient to influence the declivity directly, as the predominating, loose ejectamenta always reassert their natural slope over the consolidated flows.

It is more complicated to estimate the influence of varying strengths of the eruption. With constant small strength the cone will soon spread beyond the limit of the dropping particles and will then end abruptly against the original land surface. A stronger eruption would throw particles beyond this foot and thus form a gradually thinning flange round the original cone. Strong winds during a weaker eruption of fine ejectamenta must have the same effect,

From the foregoing speculations the conclusion may be drawn that the greater the variations both in the nature of the materials and the strength of the eruptions, the more the volcanic slope must assume a concave, sweeping section.

In order to test these various influences an experimental investigation was undertaken by the present author. The results have been embodied in a paper in the Leidsche Geologische Mededeelingen (bibl. 69). The experiments confirmed the theoretical conclusions just mentioned in most respects, but an important exception was also deducted. In volcanoes of upwards of some 1000 m high the force of the most violent eruptions is not sufficient to throw the bulk of the materials far enough outwards to influence the slope of the cone. For larger volcanoes the primary constructive forces results in a nearly straight slope.

There is, however, another influence which tends to convert a volcanic slope into a concave line, namely erosion. Poulet Scrope, according to Milne, attributed the concavity partly to erosion and later, many others have recognised its influence. Escher pointed to the erosive influence of the erupted crater lake of the Kloet volcano on Java (bibl. 39). Kemmerling believes the formation of lahars (= mud flows) by rain to be more important still (bibl. 61). It would lead us too far to mention all authors, who pointed to the influence of erosion on the concavity of volcanic slopes.

Let us examine this side of our problem more thoroughly. Geomorphology teaches us that erosion by running water produces concave slopes. This tendency is greatly increased on a volcanic slope, owing to the variation in size and the looseness of the particles. The fine matter is easily washed out and carried away by running water, and the finer it is, the smaller is the rate of the current necessary to carry it, and consequently the grade of the slope down which it can be washed. This selective transport will speedily result in a perfectly even, concave slope, sweeping from the steep top of the volcano out on to the original surface in an ever decreasing declivity.

There are three reasons why the influence of erosion must grow with the size of the volcano. In the first place, the rate at which a volcano grows upwards must decrease as the height increases. To build up one more meter to a volcano, 1000 m high, four times the amount of material is required that is necessary to add one meter to a cone of only 500 m. It will therefore grow at about $^1/_4$ of the rate, if the activity is the same. As, however, the area of the larger volcano is also four times that of the smaller one, the rate of erosion would be the same for both. Therefore, the relative influence of erosion on the smaller cone must be only a quarter of that on the larger one.

To this influence must be added the factor that the higher a peak, the more it tends to attract rainfall, thus adding still further to the relative capacity of erosion.

Furthermore, as Professor Escher pointed out to me, lava-flows are limited in length and are therefore concentrated round the upper parts of the slopes of larger volcanoes without parasitic eruption points. This will also help the erosion to produce a concave slope.

It is obvious that we cannot state in a general way which of all these factors is the dominant cause of the characteristic concave slope of strato-volcanoes. Each individual case would have to be investigated separately. From the submarine slopes, however, we may gain important data for deciding whether the concave profile must be attributed principally to primary constructive forces or secondary destructive erosion.

In order to study the submarine slopes of volcanoes we can only consider young cones which rise directly from a not too shallow bottom. Deeply eroded volcanic piles and volcanoes situated on the coasts of independent islands are of no use. In fig. 27—29 sections of all the East Indian cones which appear to conform to these desiderata have been planned. Most data were taken from fair sheets of the Hydrographical Survey, lent by kind permission of Captain J. L. H. Luymes. In a few cases, as will be seen, echo sounding sections were added to the wire sounding sections. The subaerial sections were constructed from contour maps and photographs. Where accurate data were lacking the section line is dotted. As some of the slopes represent local extremes these will be omitted in the following discussion.

From these sections the following conclusions may be drawn.

The submarine slopes for the first 200 meters vary between 4° and 46°, the average subaerial slopes from 13° to 37°. In fig. 30 the dry and wet slopes are plotted against eachother. The dots show volcanoes with a regular slope. Where a circle is added the average of the whole (concave) slope is taken, the squares representing the upper part of these and of other concave slopes. We note that there is a wide scattering of the points, indicating that the wet slopes do not increase regularly with the steepness of the dry part. As the concave slopes are influenced to some extent by

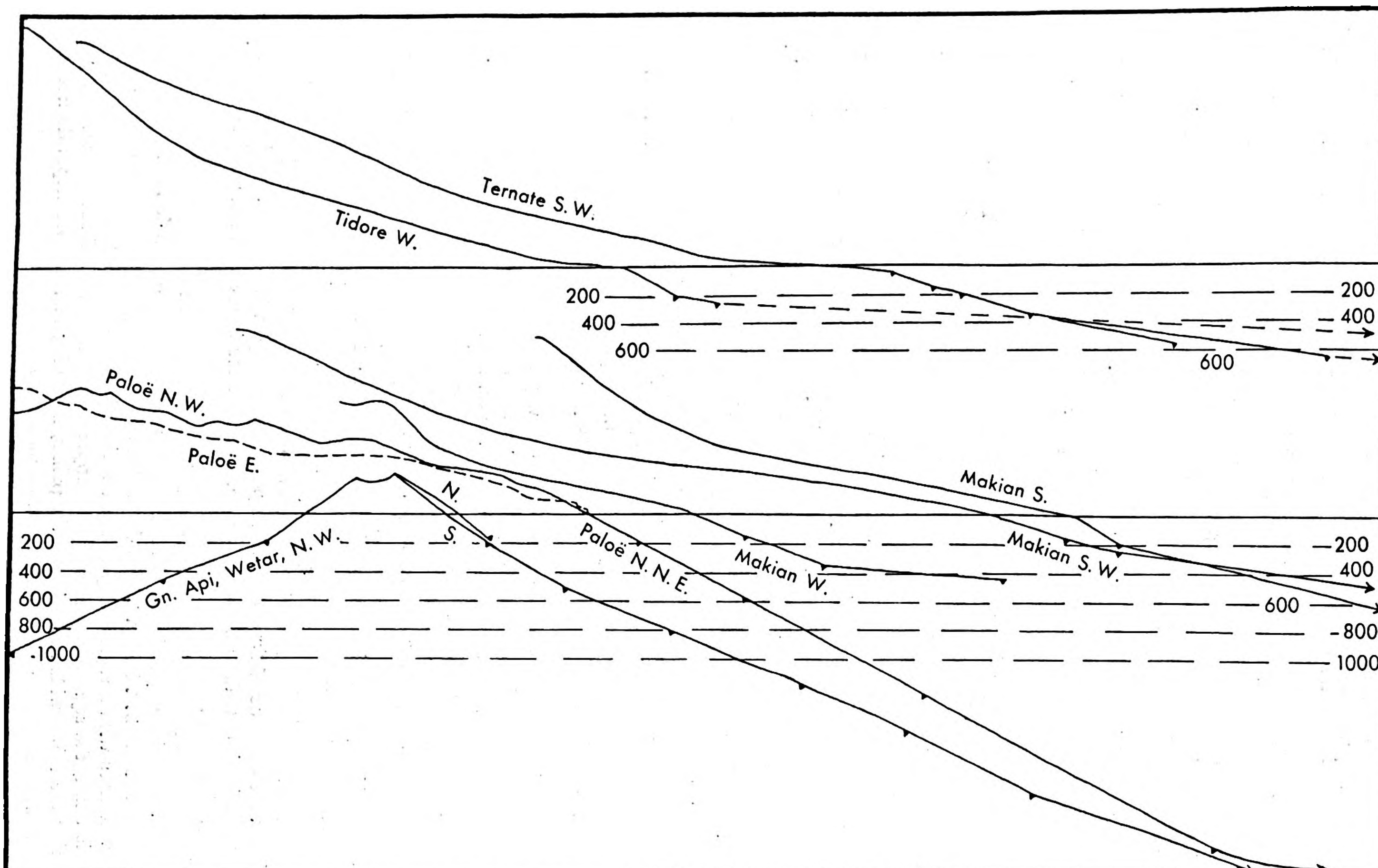


Fig. 27. Sections of some East Indian volcanoes. Soundings used are indicated by black triangles. Scale 1: 40.000.

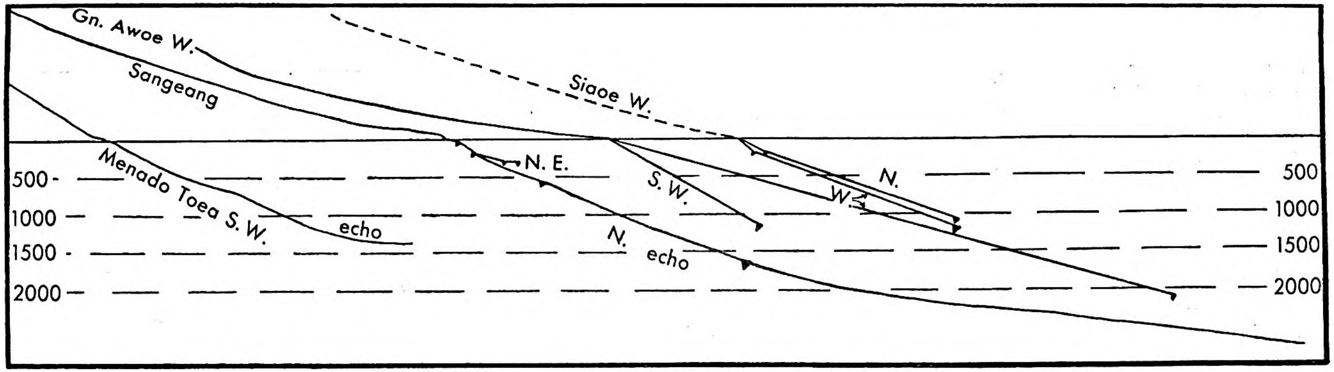


Fig. 28. Sections of some East Indian volcanoes. Scale 1: 80.000.

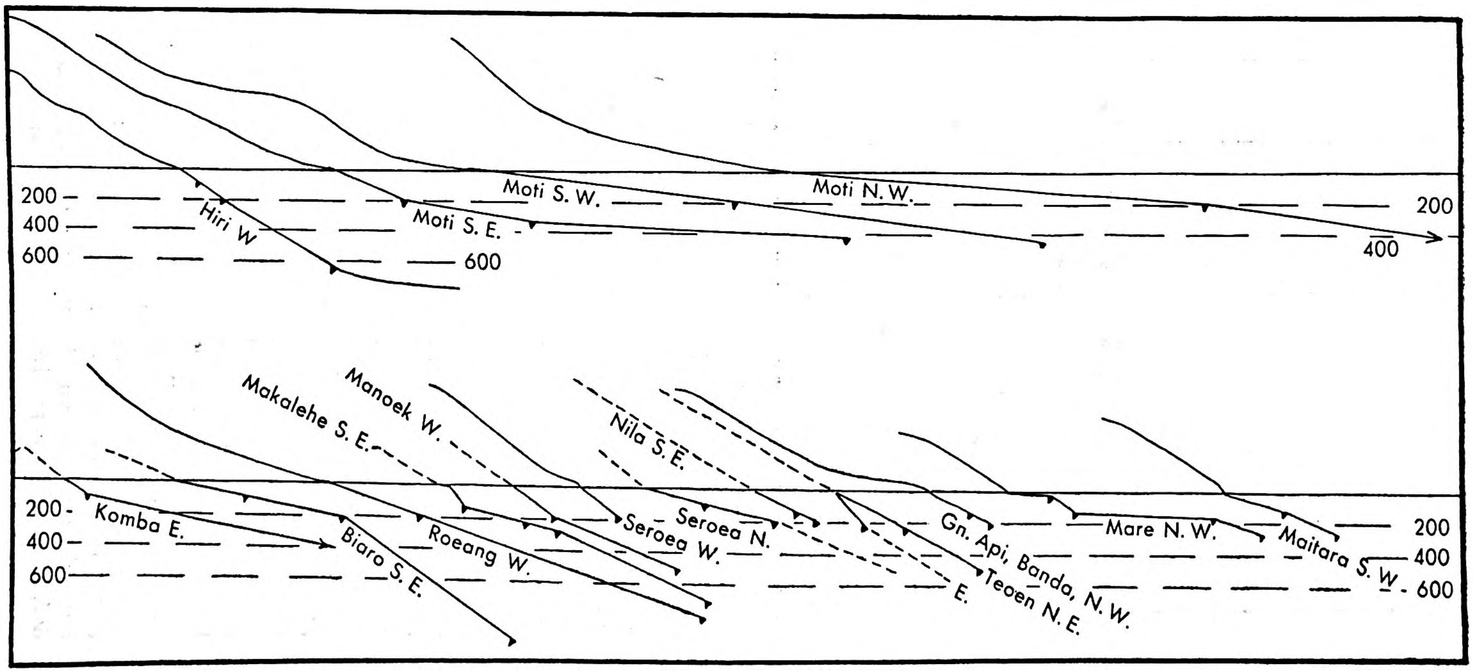


Fig. 29. Sections of some East Indian volcanoes. Scale 1: 40.000.

erosion, only the upper part (squares and dots) are of importance for a comparison. If we only consider these it becomes evident that there is a general tendency for the steeper volcanoes to have steeper submarine slopes. The dotted line shows the rough average and clearly indicates this tendency, It is evident that the influences which govern the slopes of volcanoes, such as viscosity of the lave, size and shape of the ejectamenta, and type of eruption, tend to form the same declivities above and below the water. If this rule were strictly followed the line of averages would run straight to 0°. The observed divergence of the line would mean that the declivity decreases more below than above the water. The points are, however, too widely scattered and too few in number to attach much importance to this apparent rule.

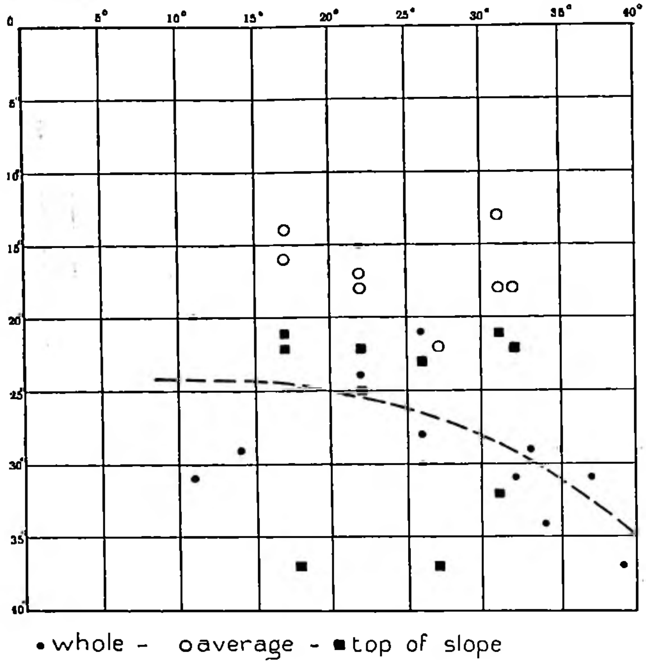


Fig. 30. Submarine slopes of the East Indian volcanoes for the first 200 m, plotted, horizontally, against the subaerial slopes, vertically.

Our next question is whether the height above sea level has any influence on the declivity of the submarine slope. In fig. 31 these two values are placed against each-other. The points are widely scattered and if we calculate the average positions for the 7 cones of 600 m and less, it is found to be 25°, for the 12 cones between 600 m and 1200 m it is 26½°, and finally, for the 8 of 1200 m and more it is 24°. Evidently the height of the dry part has no influence on the submarine slope.

The large cones are less steep on the lower slopes than below sea level. In fig. 32 the angle between these two parts of the slope is placed against the height. When the steeper declivity is below the surface a negative sign has been added, otherwise it is positive.

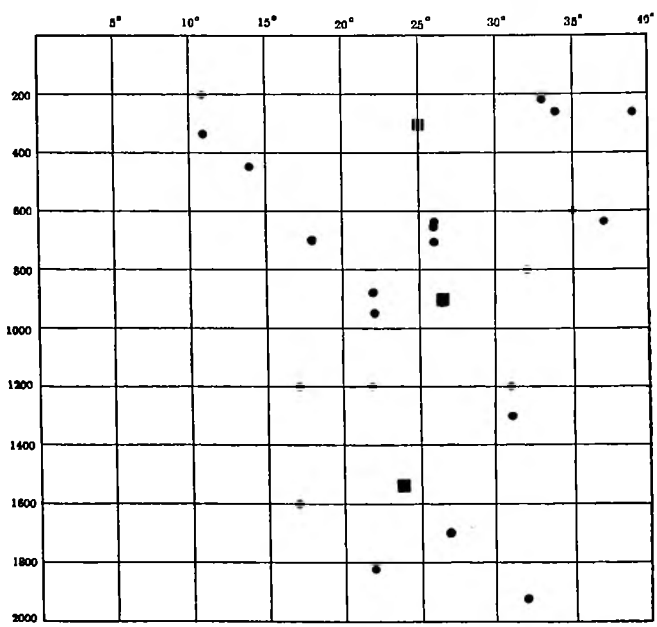


Fig. 31. Submarine slopes of the East Indian volcanoes for the first 200 m, plotted against height above sea level. Averages in squares.

In the graph thus obtained there is only a limited scattering of the points. The dotted line represents the rough average. All volcanoes lower than 600 m, with one exception, are less steep below sea level, all those higher than 1000 m are steeper, with a constant increase to approximately 22° for cones of about 2000 m. A theoretical maximum would be attained when the dry slope is 0°, The bend would then be equal to the declivity of the submarine slope. This slope averages 25° and we actually see that the line in our graph tends to approach asymptotically to a line close to 25°.

An explanation has still to be given why the smaller cones are less steep below than above sea level. As the force of gravity would have less influence on a lava flow under water and it would solidify quicker, it would have the opposite effect to that observed. It must therefore be due to the deposition of the loose particles that the slope decreases under water. Loose particles have a lower maximum angle of declivity under water than above water. Where ash and fine sand form the bulk of the volcanic matter produced, they will accumulate in a more gradual slope under the surface of the sea than above is.

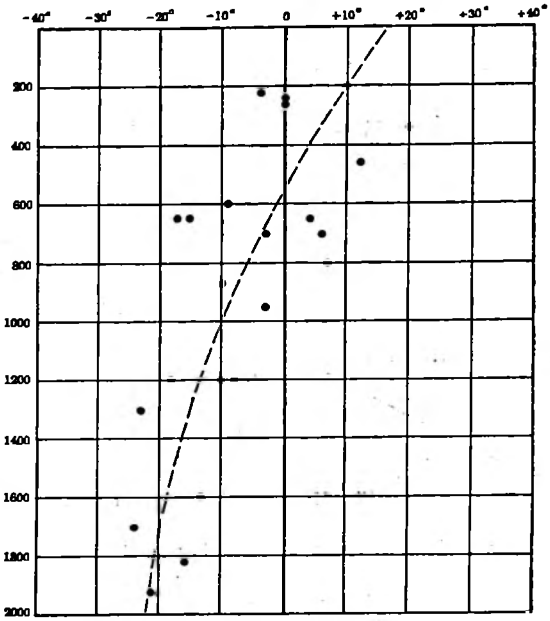


Fig. 32. Angle between the submarine - and the lower part of the subaerial slopes of the East Indian volcanoes plotted against the height above sea level.

We can now return to our original problem of finding which influence predominates in the production of the concavity of volcanic slopes. As the foot of many volcanoes has a declivity of only a few degrees and as the submarine slopes show that the finer particles, even in a wet condition, have a greater natural slope, the variations in size and shape cannot explain more than a fraction of the concavity. There remain as possible influences either erosion or the variations in the distance the particles are scattered by the eruption.

If the latter influence predominated the submarine (wet) slope might be more gradual, on account of the smaller natural slope of wet materials as is actually the case with smaller cones. The varying force of the eruption could, however, not form a slope in which the sea level had any other influence. The observed sudden and considerable increase of slope below sea level cannot possibly be explained by variations in the distance of scattering, for the distribution of the amount of material is governed by influences in the atmosphere from which it drops in regularly decreasing quantities further away from the vent. There is no reason why the amount would suddenly change beyond the coastline.

The influence of erosion, on the other hand, is intimately connected with the position of sea level, in fact, it cannot extend below it. Above and below sea level we have the influence of primary scattering, while erosion is limited to the subaerial slope. A sudden increase of declivity at sea level must, therefore, be attributed to erosion only. *As the submarine slopes are of the same order of steepness as the top of the subaerial part, the concavity of the dry slope must be attributed almost entirely to erosion.*

To test this hypothesis we must again refer to the arguments, that the influence of erosion grows with the size of the volcano. The higher the volcano, the more pronounced must be the concavity of the slope. This is actually found to be the case. It may be seen directly in the sections of fig. 27—29.

We are now able to visualise the growth of a volcano on a comparatively deep sea bottom. At first the cone grows in steepness, height and size. Soon the upper portion assumes the natural slope. This will grow up- and outwards, until over the whole pile the natural slope predominates. With

the varying force of eruption a slight concavity may result. When the cone begins to grow up above sea level it will be slightly steeper, on account of the dry nature of this part. With increasing height erosion continually gains in relative importance. On an average it will already compensate the difference between the wet and dry slope when the cone rises to 600 m. The upper part loses more and more of the fine ejectamenta and the lower parts are built up in ever increasing degree by water-borne material. The submarine slopes are fed by a decreasing quantity of direct ejectamenta and more and more by the erosion products. Waves tend to spread this delta-like material evenly over the entire circumference. The particles added to the submarine slope will also become progressively finer in proportion to the decreasing slope of the foot of the volcano. In fig. 33 an attempt is made to show the successive stages of development.

A final test of our deductions is the following. If the concave shape of a volcanic slope is caused principally by erosion, while the constructive forces result in a nearly straight slope, then the submarine slopes which are formed without interference by running water should be much less bent and the characteristic volcanic profile should be replaced by a nearly straight line.

It would not be a fair test to examine all the sections given in fig. 27—29. Many of these belong to cones built upon a fairly shallow sea bottom. Gn. Api (Wetar), Paloë, Sangeang, Siaoe and

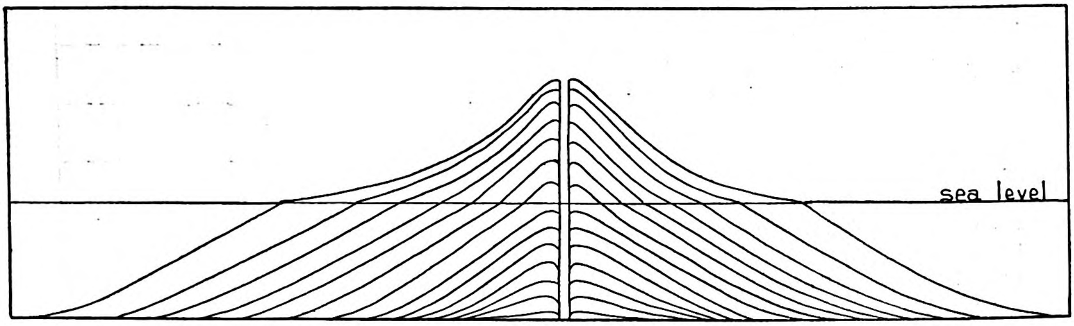


Fig. 33. Theoretical stages in the growth of a volcano from below sea level.

Awoe, Teoen, Seroea, Roeang, Biaro, Komba, Manoek, are probably the only examples we may use. Here we see that as a rule the slopes are approximately straight or very slightly concave. None show the typical volcanic slope, as represented by the dry cone of Tidore, Makian, Moti, Roeang, Awoe, Gn. Api on Banda.

As the primary scattering of the material will result in a rounding off of the angle between cone and substratum we should expect to find some indication of this effect at the bottom of the slopes. This is actually the case, as may be seen especially on the slopes of volcanoes placed in shallow water: Moti, Makian, Tidore, Ternate. The volcanoes Menado Toea, Hiri and Sangeang are further illustrations. It should be borne in mind, however, that the nature of echo soundings makes it doubtful whether the rounding off is not an acoustic deception. Landslides, the result of earthquakes, may also account for part of the rounding off, as well as the rolling down of larger blocks.

Some minor features are worthy of note. The peculiar slope of Makalehe is caused by the formation of a coral reef on the coast. The abrupt change of angle at about 200 m depth on several slopes is more apparent than real and is caused by the greater number of soundings on the shallow side of the 200 m line. The change might be found to be at a deeper level, if more soundings in greater depth were available.

Submarine volcanoes. Several steep, symmetrical elevations were discovered by soundings which may be attributed to submarine volcanoes. Three of these are given in fig. 34. Those to the east of Sangeang and to the east of Paloë are only known from one section, but their shape and position make almost certain, the explanation proposed here. The one west of Ambon is known from several sections and is certainly a fairly steep, isolated cone. It probably represents the continuation of the volcanic arc of the Oeliassers and has therefore probably been long extinct. Another steep

elevation of the sea bottom is found to the west of the Sangihe Islands (P. IV, section 14); its explanation is rather doubtful.

The apparent concavity of the slopes of these submarine volcanoes may be partly due to acoustic deception, but is probably partly caused by primary scattering. It must be admitted that it is more than would have been expected from the foregoing conclusions, as the submarine eruptions probably seldom rise to great heights into the atmosphere.

8. SLIDING OF SEDIMENTS.

Starting from two entirely different points of view two groups of investigators have proposed theories in which unconsolidated sediments are believed to slide down submarine slopes.

A. Sliding in connection with sedimentation.

The principal advocates of the first group are Arn. Heim (bibl. 50 and 51, p. 20—22), Horn (bibl. 58), Hahn (bibl. 47), Escher (bibl. 38), Hadding (bibl. 46), Shepard (bibl. 99 and 100), and others. They maintain that fine sediments accumulating on a slope are in labile equilibrium and are inclined to slide down now and then during the process of sedimentation. If the sliding takes place over a small distance and the cohesion of the material is large, finely plicated strata will result between normal unfolded layers. Hadding gives illustrations of instances in which rolled balls were even formed. Otherwise, the sediment forms a thin emulsion and spreads out further down the slope in a new layer that is not distinguishable from a normal, directly deposited stratum. Hahn pointed out that many intraformational conglomerates and breccias can be explained by long distance sliding or sliding of partly hardened deposits. He also illustrates intermediate cases in which broken

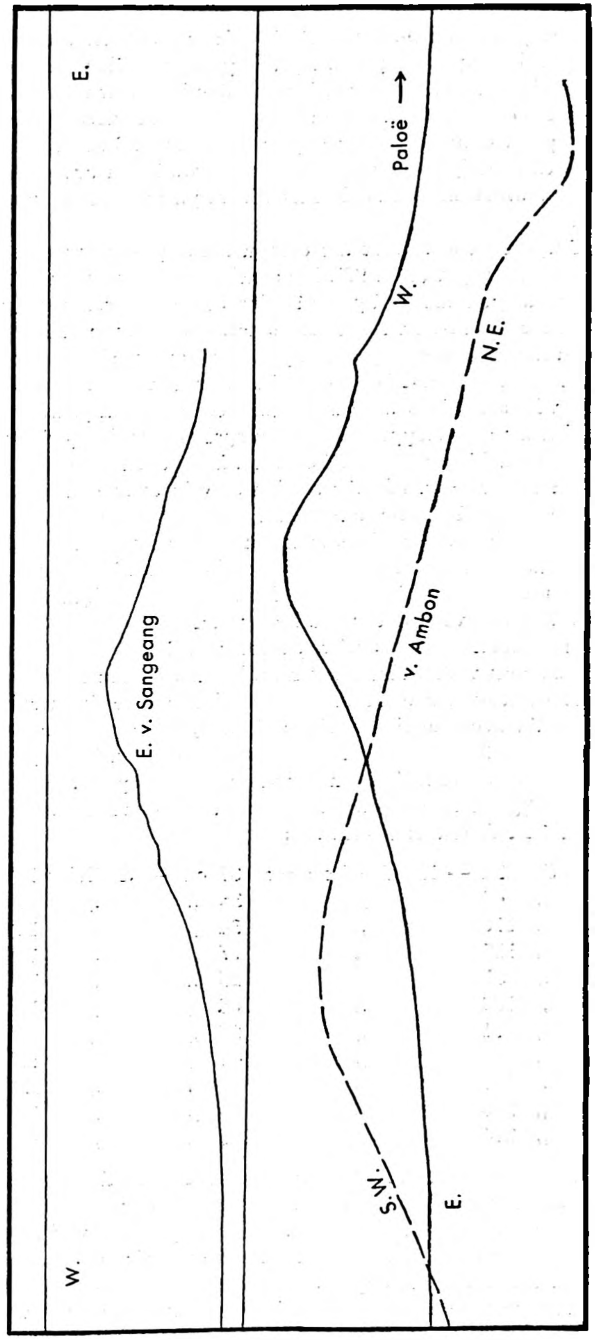


Fig. 34. Echo sounding sections of probable submarine volcanoes. Scale 1 : 80.000.

plications occur. According to Horn and Escher, the sedimentation is swifter in the shallow regions on the slope of a deep-sea trough than further out and the declivity must necessarily increase until sliding ultimately takes place. By this process shallow water sediments will be deposited in deep water and a geosyncline of considerable depth may become almost entirely filled by neritic deposits.

Heim considered that the sliding might take place on declivities of only 4.4 % (= 2½°), but Escher pointed out that the examples of sliding on Swiss lake shores, given by Heim, were all caused by the extra weight of buildings or earth works on unconsolidated deltas. He concludes that the angles at which marine sediments will begin to slide are not known, but must be comparatively small.

Shepard believes that the continental slopes are formed in part by the sliding of sediments on the steep slopes (bibl. 99). He considers that the great submerged canyons off many coasts are cleared out periodically by the sliding out of the sedimentary contents (bibl. 100).

The combined echo soundings and bottom sampling of the Snellius Expedition offer a fine opportunity of measuring the angles at which marine sediments may accumulate.

In order to gain definite data the declivity was measured at all the stations of the expedition at which wire soundings were made. The echo soundings were always carried on up to the moment at which the ship stopped and began again immediately the course was resumed, so that accurate data are available for nearly all stations. It should be borne in mind that the actual slope is as small as the slope measured in the section only in cases where the sections are at right angles to the depth curves. This certainly more than compensates the small errors which may result from inaccuracies in the soundings which in some cases might give too large a declivity. For the same reason the greatest declivity was taken in cases where a difference is found before and after the station, the smaller angle resulting from the more oblique position of the section with regard to the slope. The following data were thus obtained.

In most stations the declivity is less than 1° and the average is less than 2°. There are, however, many stations with a fine bottom sediment with much greater declivities.

If we leave out of account the stations where the proximity of the coast or strong currents prevent the accumulation of fine sediment, there are about 300 stations left for our investigation.

At these 300 stations a soft bottom was observed in all instances but 2. In 44 cases these soft, fine clays or oozes had accumulated on a declivity of 7° or more. In no less than 27 instances or nearly 10%, the declivity was 10° or more. In 8 cases it was 15° or more. In one case it was 22° and in one 34°. A few examples are:

St.	34	— length of sample	87	cm	— declivity	17°	— depth	1550	m
,,	40	,,	35	,,	,,	15°	,,	1150	,,
,,	110	,,	34	,,	,,	11°	,,	250	,,
,,	115	,,	77	,,	,,	11°	,,	2100	,,
,,	181	,,	55	,,	,,	12°	,,	2875	,,
,,	189	,,	168	,,	,,	12°	,,	1900	,,
,,	201	,,	25	,,	,,	14°	,,	3000	,,
,,	205	,,	45	,,	,,	14°	,,	4000	,,
,,	227	,,	67	,,	,,	11°	,,	3000	,,
,,	266	,,	50	,,	,,	14°	,,	2700	,,
,,	374	,,	206	,,	,,	13°	,,	2550	,,

Although there is no reason to mistrust even the most extreme cases, and, as already stated, these angles are minimum values, we will leave out of account the greatest slopes, so as to arrive at the most conservative estimate. Our data prove beyond doubt that the most mobile marine sediments known, can accumulate in thicknesses of upwards of 1 meter on angles of 15° in strongly seismic regions at all depths.

The opposite method of considering the data is to ascertain if a hard bottom was found anywhere, where the factors ruling the sedimentation would cause soft material to be deposited. Out of 300 possible localities only 2 show a hard bottom. One, station 270, may represent a case where the sediment had slid off, for the bottom shows a slope of 11°. But there is another case, station 321, where there is a perfectly flat bottom on the floor of a trough and at which a „hard bottom" was

also found. For the latter case one must assume that some hard object stopped the sampler and for the other case the same may therefore have occurred.

From the foregoing one may conclude that in the normal course of events slopes of 15° and less are not too steep for the accumulation of sediments.

It is of course possible that on such slopes sliding does occur, but that the lower layers remain in situ, while only the superficial deposits slide away from their foundation. Our investigation therefore only proves the possibility of a certain amount of accumulation, but does not exclude the possibility of slides, either sudden or gradual, at the stations. I will presently point out some reasons however, for doubting this.

Another phenomenon that might prove that a considerable sliding down of sediments does take place is the following. At the bottom of steep slopes we would find sediments that could not have accumulated in such great depths: sediments with a high percentage of carbonate would occur in depths where the normal sedimentation would only form deposits with a trace of lime. The preliminary examination of our samples has not revealed any instances of this. In the Sulu Sea, sediments, rich in carbonate, occur at depths of 5000 m, but they are formed in situ, the water of the bottom being almost stagnant and therefore not able to dissolve large quantities of lime. We must await the final examination of the samples before this question can be adequately dealt with.

As was pointed out above, it might be supposed that sliding could occur on smaller declivities, but only after a greater thickness had accumulated. This, however, is improbable as I observed that the samples became firmer and less plastic deeper down in the sediment. We should also find sharp layers where new deposits had formed on old sliding surfaces. I do not believe there are any examples of this in our collection. Where layers occur they appear to present gradual transitions.

Finally, we must bear in mind that a slope along which sediments could slide down to great depths must show a continuous declivity all the way down. A horizontal or nearly horizontal interruption of any length would stop the downward movement. All slopes with such features are therefore not ripe for long distance sliding. By consulting the echo sounding sections it will be seen that these are more the rule than the exception.

As more or less continuous slopes with an average declivity of more than 10° are very rare, the localities where sediments could slide down from neritic depth to depths of several thousand meters are therefore very exceptional.

To sum up, we have come to the following conclusions:

Although the East Indies form a region of marked submarine relief, the sliding down of superficial layers of sediment play only a subordinate part in the sedimentation which takes place in the deeper parts of the troughs.

Of course the number of samples is not sufficient to answer the question, whether sliding does occur as an exceptional phenomenon. But it does appear reasonable to conclude from the available data that Heim and Escher attribute too much importance to sliding under marine conditions.

Heim arrived at this conclusion, principally on account of the relative frequency of the sliding phenomenon in lakes. The question arises why the frequency should be greater in lakes than in the sea. The answer may be that marine sediments are firmer to begin with (partly because sea water is an electrolite) and accumulate with such extreme slowness that there is more occasion for the diagenetic consolidation of the materials after deposition before a sufficient amount has accumulated to start sliding.

In my opinion the scarcity of cases so far observed of fossil sediments that have evidently undergone sliding, is not only caused by lack of evidence and accurate observation, but also by the rareness of the phenomenon.

The principle laid down by Escher, of sliding in geosynclines, is not rendered invalid by these observations. If the amount of detrital matter brought out into the neritic zone were increased, or if sedimentation could continue uninterrupted by tectonic activity for a long enough period, sliding must eventually set in. The explanation of the intimate association of deep sea deposits with shallow water sediments can still be maintained. On the other hand, as we have already stated, in the opinion of the present author the principle is not of universal importance and many geosynclines that at some time of their history possessed great depths, were filled up and ultimately folded without any appreciable sliding of sediments ever having taken place.

There are four exceptional types of submarine slopes on which a constant downward movement of sediment does take place, namely on coral reefs, on volcanoes, on deltas, and on the slopes of more than 15°—20° (see Plate IX). The type of movement is slightly different from that imagined by Heim and Escher, for the sliding takes place immediately during the sedimentation as individual particles and not later on in a massed movement. Now and then a combined movement of already deposited particles may occur, so that a transition takes place to the movements described by Heim, Escher, etc.

B. Sliding in connection with tectonic structure.

The second group of investigators who believe that sliding movements of sediments occur, hold that some or all of the folded or thrust structures of mountain chains have been produced by the sliding down of thick strata of unconsolidated sediments.

The former group believes that the tectonic structures are produced afterwards by horizontal compression.

The principal advocates of this school of thought are Reyer (bibl. 90), Abendanon (bibl. 1), Haarmann (bibl. 45) and van Bemmelen (bibl. 9, 11—14).

In my opinion, these authors have overshot the mark. In drawing attention to the influence of gravity on tectonic phenomena they have certainly opened out or re-emphasized an important line of enquiry. In attempting to explain all tectonic activity by sliding, however, they act as someone who would try to attribute all waves to disturbances by movements in water of solids, and none to wind. Only by studying the theory of waves and by analysing each case could an answer be found to the question of the origin of each example of waves. In the same manner, a careful study of nature and the consideration of differences in the structure as a result of folding through compression or through sliding, may bring out the true relative importance of both. To start out by postulating that sliding is the sole — or most important — cause can only retard the solution of the problem.

We will not consider the merits of the various theories that have been advanced and the possible objections which can be made to them, but we will attempt to throw some light on the general principle by the examination of our new data.

There are three sides of the problem that we must inquire into, namely:

1. The relation between the slides advocated by the above-mentioned group of investigators and the type of slides to be considered here;
2. The declivity of the slides;
3. The question whether sliding and tangential compression lead to exactly the same type of structures and if not, whether mountain chains give evidence as to the method by which they were folded.

1. *The relation between the „stratigraphical slides" and the „tectonic slides"*. We have learnt above that a few unquestionable cases are known in which minute slides have occurred and that some rock structures must be attributed to small slides. At first sight it does not appear unreasonable to assume that such slides could also take place on a larger scale, giving rise to structures of the magnitude of orogenetic processes. If this is the case, however, we must also expect to find slides of medium magnitude. Layers, one or a few meters thick, should also slide now and then. Hahn gave a clear example of a medium sized slide in the strata of Trenton Fall (bibl. 47) and de Terra a fairly convincing one from Wyoming (bibl. 114). Picard describes some from Palestine (bibl. 87). Van Bemmelen has illustrated fine examples of such structures in volcanic tuffs from Sumatra and Ischia (bibl. 10, p. 63). The resulting structures are indeed so characteristic and so obvious that if the phenomenon of slides of medium size were common it should in many cases be extremely easy to detect them, as they are local in a horizontal and vertical sense and of sufficient size to be seen and show the internal structure. The almost complete absence among normal folded strata of such structures, which would have puzzled tectonic geologists in no small degree and could therefore hardly have escaped general observation, warrants the conclusion that medium sized slides are relatively rare. This scarcity of intermediate instances is certainly not favorable to the theory that sliding on a larger scale is the cause of the universal phenomenon of folded mountain chains.

2. *The declivity of the slides.*

Haarmann is very vague on the question of the declivity required for gliding to take place, on

account of the absence of trustworthy data (bibl. 45). As this point is most intimately connected with the fundamental principles of the theory of folding by sliding, it is nevertheless of importance to gain at least some insight into possible angles. It would appear from the following quotation that Haarmann believes a very slight slope to be sufficient: „Die Senken und rückenformigen Erhebungen des Meeresbodens haben dort, wo weiche Sedimente vorhanden sind, selten Böschungen von mehr als $^1/_4$° . . ." „Wo festere Gesteine auftreten, finden wir steilere Neigungen: 10° bis 16° und mehr. So hat der Yapgraben 18° bis 19°". (Haarmann, p. 114).

That firm rocks can sustain much steeper slopes was shown by the present author in connection with the submarine slopes of coral reefs (bibl. 67, p. 95). Slopes of 50° over 600 m were recorded. On submarine slopes of volcanoes declivities occur of 34° over 1000 m. Moreover, we note the occurrence of vertical drops of 800—1000 m and local steeps of 26° over 2500 m, of 30° over 2100 m and of 36° over 1000 m on various parts of the sea floor.

On the other hand, when submarine landslides of hard rock do occur (see Cape Kantaboeria[1]) on Boeton, bibl. 67, p. 62) they are a purely local phenomenon, comparable in every way to the same formations on dry land. They could not possibly lead to tectonic structures.

When considering the case for plastic sediments our problem is complicated, for the thicker the deposit, the smaller the angle need probably be at which sliding could begin. We should therefore have to know the thickness of the mobile sediment on the submarine slope, in addition to the angle of the slope. Here our data are as yet of little use. The table on page 70 gives us the length of the sample of the stations under consideration. As the sampler always penetrated further, the minimum thickness of the sediment is greater than the length of the sample procured. The most extreme cases are those of stations 189, and 374, where the sampler penetrated about 3 meters and the slope was 12°—13°. The minimum thickness of 3 meters is too small to be of much use for our problem, as in folded mountains the strata are seldom less than several hundreds of meters.

In the same manner, little aid can be obtained from the nature of the sediments at greater depth in the bottom. It was very regularly observed that the deposits became firmer in the lower end of the samples. Even in the samples of 1½—2 meters in length this gradual loss of plasticity with increasing depth below the surface of the deposits was very marked. This diagenetic hardening of the sediments had certainly much more influence on the plasticity than the contrary influence of pressure which is supposed to increase the mobility. But it is possible that at a comparatively small depth the maximum rigidity is reached. As the influence of the weight would not show a maximum, a more mobile state of the deposit could still be reached at greater depth than is shown at the surface.

Some of our steep sediments are deposited at depths of several thousands of meters, where the pressure is equal to a load of more than 1000 meters of strata. Probably the two circumstances are not comparable, so that a direct conclusion as to the influence of load cannot be drawn from investigations of the mobility at varying depths. This line of thought is therefore only suggested as a possible road to new points of view.

We therefore find that our data give little positive help in ascertaining the declivity at which sliding of thick strata could take place. On the other hand, it was shown that plastic, fine clay and ooze may accumulate on slopes of 10°—15°, in layers of more than 3 meters thick. This is about 50 times as much as the angle that Haarmann believed possible for soft deposits, namely $^1/_4$°.

One cannot help being extremely cautious when following Haarmann's further conclusions of „secondary tectonic phenomena", when we find one of the basic assumptions is connected with values which are entirely without foundation.

3. *Would sliding and tangential compression lead to exactly the same type of structures?*

A different line of enquiry is to deduct theoretically the various types of structure which would result from either sliding or from compression and also to ascertain the results of sliding in the Molukken which form the submarine region with the most intricate and intensive relief of the entire globe, so that if sliding takes place at present anywhere, it must certainly occur there.

Foremost among the differences that must result from sliding or compression is that in the former case an equal amount of stretching on the tumor must correspond to the compression in the trough. Haarmann had already pointed out that compression and stretching must be intimately

[1]) This should have been denoted by: West side of Nalandi Bay.

connected, but the instances of stretching he gives are not what is really required. The Scandinavian fjords and alpine valleys for instance, cited by Haarmann as instances of stretching, have rocky bottoms, are frequently transverse to the strike of the geotumor and the geological trend lines are continued uninterrupted on both sides. These points prove that the said features are only superficial erosion gullies, not gaping fissures. Although stretching may prevail in faulted regions, the vertical movements still dominate over the horizontal and here also, gaping fissures are absent, even in the grandest example, the East African fault-zone. No genuine and definite case appears to be known where the great foreshortening observed, even in a slightly folded chain, is compensated by an equal amount, or even a small fraction of that amount, by stretching. This has already been pointed out in bibl. 5 by several authors.

Van Bemmelen draws attention to the fact that the alpine nappes show signs of stretching, which is generally explained by the activity of a „traineau écraseur", a higher element which squeezed out the lower nappes, but that in many cases this traineau is hypothetical or absent. One must admit that sliding offers a possible way out of this difficulty. But the amount of stretching is far smaller than the amount of proved overlap, so that even if sliding aided the formation of the nappes it must have been subordinate, as far as the visible structures denote, to the actual tangential compression.

It could be advocated that the stretching is obtained by the thinning of plastic strata. But all sedimentary series contain a large percentage of brittle sandstones or limestones. These would have been broken and dragged widely apart. Neither can the entire lack of stretched zones be explained by the erosion on the geotumor. We cannot assume that in all cases the stretching only occurred above sea level. Haarmann himself emphasizes the frequent formation of folded structures at a low level. On steep submarine ridges or in the passages between islands on an arc, the stretched zone would have been preserved in many cases.

If we consider the great number of compressed zones in the mountain systems of the world and the endless variety of the forms, it appears entirely unreasonable to suppose that the stretched zones that must be of equal importance would never have been preserved.

Can the absence of stretched zones be explained by lack of accurate observation? This is hardly possible, for in many cases the structure would be highly characteristic. If we imagine a large submarine geotumor, comparable to the great ridges in the East Indies, the slopes are convex towards the top. Sliding must necessarily begin somewhere on the steeper part of the slope. The sediments higher up will partly follow through lack of support, but between these parts and the broad horizontal back of the geanticline, where no movement can set in, there must occur a zone which will show gaping fissures. These would subsequently be filled in by the crumbling of the edges, by the pressing up of the lower strata and by new sediments. Such zones would somewhat resemble sun-cracks on a gigantic scale (see fig. 35). Once formed, they could hardly escape observation when raised above sea level at a later date, even if the cracking were confined to the more brittle strata. Of course there might be many instances in which the stretched zone was obliterated later, or for some other reason, was not observed by the field geologist, but the entire absence of such structures is equivalent to proof that stretching on as big a scale as compression has not occurred in the history of the earth. This contradicts the postulates of the sliding theories.

Another difference between structures caused by sliding or compression is to be expected in the folded parts themselves.

As we have already noted above, many of the steeper slopes are not formed by an even and gradual decline down to great depths. If for some reason movement began on such a slope with steps, or even with counter slopes here and there, the result could not be a regularly folded belt or an overthrust sheet of simple structure. More intensely folded zones would lie between undisturbed and stretched zones.

Where the alterations in steepness are greater still, other complications must arise. On portions of the slope with a declivity of about 5° the sliding would not take place until after the depositing of a great thickness of sediment, while on other parts with a slope of 20° or 30° several slides would have occurred in the same period. Traversing a single section in a mountain system we would find the same geological formation to be folded only once here and more intensively and repeatedly elsewhere. Over the edge of a submarine precipice the sliding strata would fall like an avalanche

to be coverred up later by undisturbed sediments of a slightly more recent period (fig. 35).

Further, as we have already noted, on a convex slope the sliding would begin somewhere on the steeper part. Higher up would be a zone that would not move of itself, but by the disappearance of its support it would now follow in a confused rush of loosened blocks, tumbling against and over the neatly folded series below. Finally, towards the horizontal top of the slope there is a zone that has not moved, with a strip crossed by cracks and fissures. Naturally, we could imagine that the plasticity is greater, excluding the formation of actual blocks. Nevertheless, the universal alternation of plastic and brittle strata must still cause different sections to those of folding by compression.

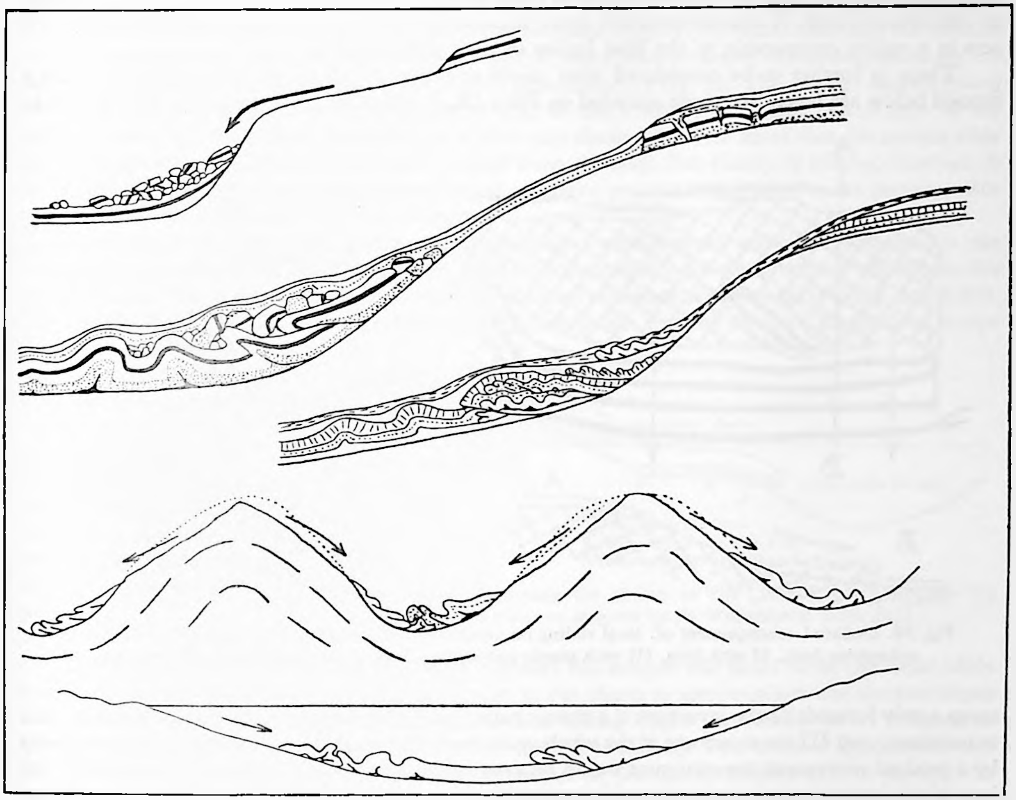


Fig. 35. Deduced concequences of the sliding hypothesis for a region comparable to the present state of the East Indies.

As in the case of the sun-cracks, these other complications need not always be obvious especially where the sliding took place slowly and gradually, but they would have been noted frequently in the course of the investigation of the countless orogenetic systems of the world, many of which must have been folded wholly or partly below sea level, as Haarmann himself emphasizes.

Then there is the case of high and narrow anticlines. The limbs of these must in some cases have been too steep for the plastic and mobile sediments to hold. Bilateral sliding of the upper strata and new deposits would have caused in such cases very curious and obvious structures.

Another type of irregularities would be found by passing along the strike. On the slope of a geanticline the amount and the character of the sediments must vary considerably and at the same time the declivity of the slope is also constantly varying within comparatively wide limits. Consequently, the whole flank of a geanticline cannot be ripe for sliding at the same period. If at a given point sliding of the sedimentary covering begins, there must be instances in which it cannot sweep down neighbouring parts of the slope that are not already in a labile condition. The plastic condition

of the strata one must assume, to make the sliding and folding possible, is incompatible with a sufficient rigidity to drag down resisting portions of the covering of the slope. Consequently, on a rising or steepening geanticline, sliding will take place now here, now elsewhere. The direction and the amount of movement would vary from section to section. Brecciated and jumbled zones would indicate the edges of the various slides where the folded and heaped up strata toppled over and spread into the unaltered zone beside it. In other cases, narrow, abrupt zones of virgation would form along the edge of a local slide. We should also expect the occasional rotation of portions around a vertical axis, placing their structure obliquely to the normal trend lines, which would possibly be influenced again by new distortions parallel to these normal lines, but obliquely to the older structure of the rotated block. A chaotic structure must necessarily result from a mechanism of sliding, if it acts in a region comparable to the East Indies in their present form.

There is further to be considered what might sometimes happen when a large fault scarp is formed below sea level, like those recorded on Plate IX, on the flank of a ridge. Fig. 36, I shows the

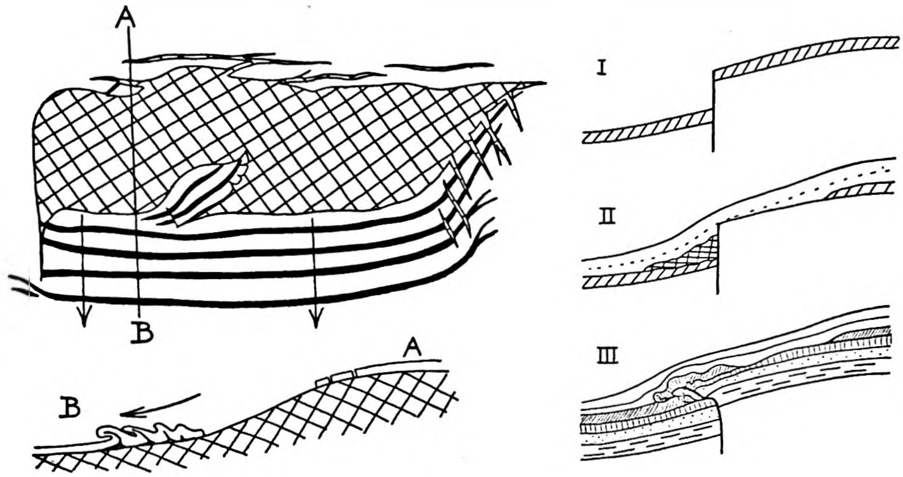


Fig. 36. Deduced concequences of: local sliding on geanticline with varying declivity; and of sliding on submarine fault, II with firm, III with plastic substratum, both with subsequent sedimentation.

scarp newly formed; II the structure if a plastic superficial layer were to slide off and sedimentation to continue; and III the structure if the whole mass were plastic. If the scarp were developed slowly by a gradual movement the structure would be even more complicated, but no less obvious. To the best of my knowledge no such structures have yet been recorded, although the existence of suchlike scarps in former periods seems most probable.

In a general way the alternation of plastic and more resistant strata that occurs in many sedimentation troughs, would have favoured the formation of phenomena comparable to epidermic folding.

If we compare these imaginary structures with those of the better known mountain chains we find a striking contrast. The synchronism of the successive movements of an orogenetic belt, the slow variations in structure with depth and along the strike, and above all, the surprising degree of uniformity of foreshortening along all cross-sections, all these typical characteristics of orogenetic belts might now and then be absent from a structure formed by sliding down.

Very few details are as yet known of the structure of Timor. It appears to be much more complicated in detail than the Alps. We can agree with van Bemmelen that in this case sliding could explain the tectonic structure. Compression, however, would also have given rise to a jumbled structure, on account of the interchange of thick, soft clays with hard, brittle limestones. Proof one way or the other is not possible without minutely detailed, field examination. We can also admit the possibility of sliding playing a greater or smaller part in the tectonic structure of the Ruhr coal

field, as Haarmann suggests. Here also, the structure is not yet sufficiently well established to be used as a clear example of the mechanism.

On the other hand, we are familiar with a great number of very accurately examined regions where regularity in stratigraphy and structure and the absence of an increased structural relief downwards are established beyond doubt and which are explained with far greater ease by the normal conservative theories of compression.

In a recent study of the structure of the Apennines de Wijkerslooth (bibl. 131) explains the development by sliding. A number of arguments are brought forward in favour of this hypothesis which certainly merit attention. There are, however, as important points against this explanation.

If we draw the section for the upper Eocene of his plate I (fig. 37) to scale, assuming the vertical scale of the original to be 1:200.000 at most (height of tumor 3000 m, depth of foredeep 3000 m), the horizontal scale 1:2.000.000, the result obtained is represented by fig. 38. The average slope is found to be roughly 2°, the maximum slope at the edge of the trough 3—4°. The component of the force of gravity in the direction of sliding would be only about 3 %. For more than 20 km the slide passes horizontally and so must have been pushed from the rear. The theory of sliding, however, is invoked by de Wijkerslooth, for the very reason that latteral pressure is believed to be incompatible with the observed structure.

It is neither clear how the gliding mass could have produced an imbricate structure in the substratum, as assumed by de Wijkerslooth; for if it is sufficiently plastic to stretch out during the sliding, it would be impossible for it to break up the firm substratum and push it along into scales.

Doubtless the theory offers an elegant explanation for the outliers of the „Liguriden" nappe and the absence of folding and plication. On the other hand serious objections are also encountered, so that it is difficult to feel convinced from de Wijkersloooth's expositions, that the gliding theory offers the sounder explanation.

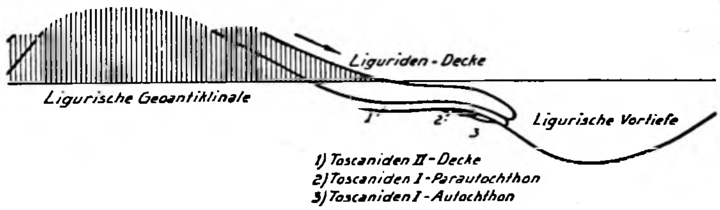


Fig. 37. The section illustrating the sliding of the „Liguriden" nappe for the uppermost upper Eocene as given by de Wijkersloooth (bibl. 131).

Of two slopes with the same sedimentary deposits the steeper one must be in the most labile condition. Bearing this in mind we may again turn to our chart, to ascertain how the steeper slopes are distributed in the East Indies and where therefore sliding might presumedly be in progress or imminent. The result is again unfavourable to the theories of sliding. This is most apparent for the Outer Banda Arc. The slope is found to be much steeper on the inner, concave side of the arc. On the Outer Banda Arc sliding would therefore occur towards the concave side of the arc, or in the opposite direction to the structure of the older folding and thrusting, that is believed to be directed towards the outer, convex side of the arc. I am not aware of instances of mountain structures which have been accurately studied, in which the direction of folding or of thrusting was reversed

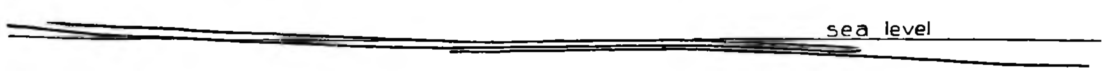


Fig. 38. Sliding of the „Liguriden" nappe after de Wijkersloooth, but drawn roughly to scale.

during the orogenetic history, nor of mountain arcs with a centripetal movement, except perhaps the south-east Carpathians (see fig. 42, p. 88).

Van Bemmelen believes that the Insubric phase, by which the roots of the alpine nappes were slightly inverted, could be explained by sliding in an opposite direction in the rear of a migrating geanticline. The present condition of the East Indies would then represent the „Insubric phase." I have already pointed out (bibl. 66) that the structure of the Bergamo Alps is directly opposed to

this view, as it is dominated by wedges with a steep upward movement. Only compression can explain this structure as one can see by studying the sections of this region (see for instance bibl. 27).

Then there is the case of the Timor-Aroe Trough. These troughs are approximately symmetrical. Sliding would result in a folded region with movements from opposite sides towards a common centre, an unknown or very rare tectonic structure. In the Ceram Trough the movements would also be towards eachother, but divided by an unaltered central region, where a low central ridge is found. In the region south of Boeroe a maze of opposed and minor folded elements, cutting across eachother, would be formed by sliding.

If we assume that sliding is the major cause of tectonic structures and then deduct the results for a given case, as was attempted in the foregoing pages, we doubtless make errors. Some of our objections to the theory may therefore be invalid, or could be met by slight alterations in the hypotheses. However, the large number of results which are in contradiction with what is known of tectonic structures, warrants the conclusion that either the theory of sliding must be abandoned as principal cause of mountain folding, or that the east part of the East Indies is not at present a region of tectonic activity. If the latter view is preferred, entirely different circumstances must be assumed to have prevailed in geosynclinal areas during orogenetic activity.

The sound reasons given by Argand, Molengraaff, Vening Meinesz and many others, for assuming that the East Indies are a region of tectonic activity, possibly in a quiescent stage, would have to be refuted. Moreover, however much the face of an orogenetic region is supposed to differ from the present condition of the Molukken, there still remain the more general objections, based upon the differences between structures caused by compression and those which would result from sliding, for these were all in favour of tangential compression. As we have already stated we need not expect to find the proof of sliding in each and every case, but there should be known many ambiguous instances and at least a few in which the features typical of sliding had been clearly observed.

It is not within the scope of this report to enter into the other aspects of the sliding theories which are less directly connected with the results of the expedition. [1]) The advocates of sliding theories in my opinion should attempt to find examples of accurately studied regions where the structures in section and ground plan and above all, in detail, show features (for instance such as those deduced above), which point to sliding and not to tangential compression. Their elaborate theories would then be better apt to convince more conservative minds.

Our conclusions are much less unfavourable to the conception that thrusting of nappes may be aided by the force of gravity, as suggested by Schardt, Schmidt, Horn and others. Even a limited continuation of the movement of thrust masses, after the orogenetic forces have ceased, seems possible. Our objections are against a sliding equal to the amount of shortening by folding and overlap, not against a slight exaggeration of the tectonic movements in rare cases.

[1]) Some other objections, especially against the undation theory of van Bemmelen, were given by the present author in bibl. 66. In answering these, van Bemmelen (bibl. 13) only touched upon a few of the points raised.

See also note on page 110.

CHAPTER V

TREATMENT OF THE TECTONIC THEORIES RELATING TO THE EAST INDIES

1. EXPOSITION OF THE PRINCIPAL THEORIES.

Readers, who are unfamiliar with East Indian geology, may find a summary of some theories on the structure of these parts and its mode of development of aid in following the later detailed treatment of a number of points, calling for comment in connection with the submarine morphology.

The varied opinions which have been expounded on the development and nature of the island arcs and deep-sea basins and throughs of the Indo-Australian region are manifold. Whilst some authors have essayed to explain the entire region, others have confined their attention to particular features or smaller sections.

The earlier conceptions were naturally based on scant data concerning the structure of the islands and a very poor knowledge of the morphology of the sea bottom. It is unnecessary to name all the authors separately, for the reason that their opinions are only of historical interest. The deep troughs were considered to have sunk, and the islands to have risen along faults.

A more recent group of theories draw a parallel between the East Indian chains and the continental orogenetic structures. These regard the archipelago as a normal orogenetic belt, which may in the future develop into a structure similar to that of the Alps.

Molengraaff was the first one to expound more modern thoughts on the tectonics of the Molukken. The observations that were made on his renouned Timor Expedition led him to the following conclusions.

In the Miocene a period of intense compressive folding in Timor and the neighbouring islands of the Outer Banda Arc caused the formation of imbricate structures and two or three overthrust sheets. He states (bibl. 83, p. 689): „Thus one might suggest that the resistance, or rather the underpressure of the Australian continental block, which was not affected by the Miocene folding, caused the overfolding and overthrusting towards the south south-east in the Timor range." (Independently Wanner and Weber came to the same conception of an overthrust structure in other parts of Timor). In a subsequent period, which is probably still continuing at the present time, block-faulting raised the islands to their present heights above sea level. The deep troughs lying to the north and south were at the same time faulted down. This he deduces from the fact, that Timor and most of the other islands are fronted on both sides by deep depressions and faults, formed posterior to the main orogenetic period. Evidently the elevation and depression are contemporary processes. These faults frequently cut the old trend lines at sharp angles and cause the folded mountain ranges to end in successive bluffs along the coasts. Owing to the same cause the north and south coasts of Timor are devoid of reef terraces. The reason for the development of the island ranges and elongated troughs is a deep seated compressive folding in large, broad anticlines and synclines. The trend of these major tectonic anticlines is the direction of the island arcs, while the great synclines lie beneath the deep-sea troughs. The passages running between the elevated islands are formed by faults sometimes aided by pitch.

The Inner Banda Arc was traced to Banda and thence onwards to the Siboga bank in a later publication (bibl. 85). The Outer Banda Arc was drawn from Soemba to Boeroe. Molengraaff suggested that it might be continued in the Toekangbesi Group. This group was possibly the continuation of the southeastern arm of Celebes, but was deflected to the northeast along a fault traced by the Boeton Deep. The further continuation was to be looked for in the northeastern arm of Celebes.

Soon after Molengraaff and Wanner had discovered the intensive tectonic compression of Timor and its neighbours, *Argand*, in his famed article on the western section of the Alps drew a most important parallel between the embryonic stage of the Alpine orogenetic belt and the present East Indian structure (bibl. 3, p. 179).

The new detailed analysis of the stratigraphy and structure of the Alps, showed that, since the earliest stages following the carboniferous mountain building, two major anticlines have developed along the bottom of the geosynclinal Tethys. These elongated and curved elevations grew in importance and height till they emerged above sea level. This already occurred in the Triassic. The development of the geanticlines progressed till these finally formed the frontal parts of the great nappes of the Grand-Saint-Bernard and the Dent-Blanche. The nappe of Monte Rosa was formed in between during a later stage of development and never attained the same degree of structural importance. The geanticlines were steeper on the outer, convex side where they discharged considerable amounts of coarse detritus which was never deposited on the more gentle slope, on the concave side.

The other geanticlines and intervening troughs were of much less importance and were the outcome of warping of the foreland and hinterland. The general explanation for these happenings is sought by Argand along the following lines:

The approach of the African and European Continents compresses the contents of the geosyncline. The plastic matter is forced to adapt itself to the shape of the concave edge of the Hercynian continent and flow out radially. Alone by the assumption of great plasticity, can we explain the divergence of the direction of flow from the direction of thrust which attains 90° and more in the Maritime Alps. A less important rigidity is responsible for the virgations and the more marked outward bending between resisting portions of the foreland where the direction of thrust is parallel to that of the flow. In the western Alps the direction of flow to the west is at right angles to the direction of thrust coming from the south; here consequently it is only the plastic flow and not the rigidity that directed the development. The remaining influence of the rigidity is to be seen here in the thinning out of the structural elements. The shape of the chains is compared to waves entering into a bay and curving round so as to accommodate their shape to the direction of the beach, without, however, attaining complete parallelism to it.

In comparing the embryonic stage of the Alps to the East Indies, Argand places the map of the latter region upside down, because the concave side of the arcs points to the north. Although, as Argand points out, the comparison should not be carried too far, there is a surprising degree of similarity between the two structures. In both there are two major geanticlines with an arcuate plan. These have emerged and spread detritus in the neighbouring troughs. The Timor Trough represents the „geosynclinal dauphinois et valaisan." The Weber Deep corresponds with the „geosynclinal piemontais"; both being deeper than their outer „neighbour." The straighter shape of the Inner arc from Flores to Sumatra recalls the eastern Alps, with their less obstructed development.

In a later publication, Argand followed Wegener's explanation of the island arcs (bibl. 4, p. 296, 321—324). These conceptions will be treated further on, p. 98.

Brouwer developed ideas similar to those of Molengraaff and Argand on the structures of the Molukken in a series of publications, working out details and regarding the entire region from one standpoint. Brouwer's point of view is characterized by the emphasis which he lays on the importance of the horizontal movements of the geanticlines. He further insists that the present development is the normal continuation of the tertiary mountain building (see bibliography).

The Banda arcs first consisted of two parallel, regularly curved geanticlines. These culminations moved towards the Australian Continent. The outer arc was forced to deviate from its original simple shape, assuming with increasing exactitude the shape of the obstructing continent. The inner arc has developed unobstructed and still presents the simple curved shape.

The great compression of the outer arc resulted in the formation of overthrust sheets, especially

in the region around Timor, where the continent was first reached. The volcanic activity on the inner margin of the arc was cut off from its source by the thrusts and the thickening of the crust. This occurred before the islands had risen above sea level, for the lavas were extruded by submarine volcanoes (N.W. Timor, Ambalaoe, Ambon, Noesa Laoet). The inner arc is still characterized by active volcanoes that have built up islands rising above sea level. The setting of these on the geanticline is a consequence of the stretching that must occur on the crest of the anticline.

Brouwer made a very important observation in this connection (bibl. 18). Between the islands Pantar and Damar, active volcanoes are unknown. It is, moreover, found that in passing along the arc from these points towards eachother, the elevated reef terraces, reach up to increasingly higher levels. As the terraces must have been formed and elevated since the extinction of the volcanoes, the period of extinction must have been farthest back for the centre of the dormant part of the arc, approaching the present period more and more on passing outwards towards the active parts. The amount of denudation of the extinct volcanoes tells the same story. The higher the reef terraces lie, the less well preserved is the conical shape of the volcano. It is a marked coincidence that the dormant part of the arc is also that portion that lies most closely up against the outer arc. Brouwer

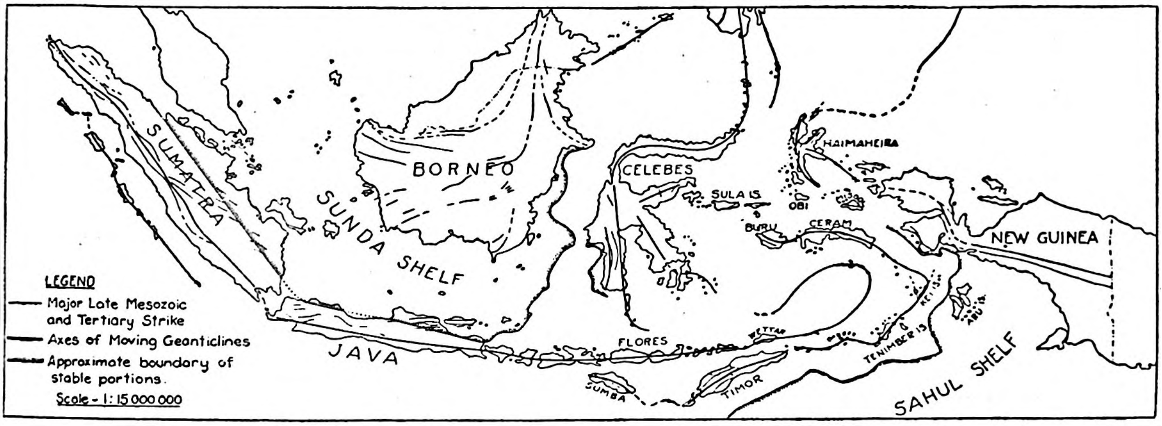


Fig. 39. Structural map of the East Indies by Brouwer, bibl. 24, fig. 1, p. 58.

concludes that the compression of the arcs has caused the volcanoes to be cut off from their magma chambers probably by the formation of thrusts as well as by simple compression. This elegant theory also explains why the extinction spreads slowly from one centre in both directions along the arc.

According to Brouwer the horizontal component of the movement of the geanticlines is proved by the following observations.

1. The Tanimbar- and the Kai Islands show a sweeping outward bend opposite the deep bay in the Sahul Shelf. The geanticline was able to move onwards further into this space, thereby obtaining a less uniformly bent shape than the inner arc.

2. Asymmetrical reef caps are found on the portions of the arc bending outwards. Roti and Jamdena provide the most striking examples. Brouwer does not explain the asymmetrical shape simply by greater elevation on the highest side of the island. In his opinion the culmination moves onwards like a wave below a sheet of ice. The rocks of the island are first elevated on the outside of the geanticline, sinking away again after the culmination has passed beneath them. The estuaries on the northwest coast of Jamdena may be a consequence of this sinking. It is the asymmetrical shape of the moving geanticline which causes the asymmetrical shape of the reef cap.

3. The strike of the strata indicates the direction of the former anticlines. Since then, the geanticlines executed differential horizontal movements, in consequence of which the strikes are cut off, obliquely, by the present coast lines. This phenomenon is most noticeable at the bending points in the present geanticlines (Babar, Morotai). These bends were thus produced by differential horizontal movements which occurred after the tertiary folding, fixed in the strike.

4. Numerous horizontal transversal shift-faults are proof that horizontal movements actually take place. Brouwer, however, believes that they merely represent the *differences* in horizontal movement and constitute but a fraction of the total displacement. He discusses a number of examples where the geology of adjacent islands indicates considerable displacement by faults.

On the whole, the larger islands are elevated the most, but a number of small and high islands exists as well. These cannot be the result of pitch of the geanticline, but they were uplifted along faults (islands of the Babar Group). The Sermata Islands show abrupt changes in geological composition from one island to another, while Kisar of similar build to its neighbour — Leti — is situated a long distance behind its geological continuation. These anomalies find a satisfactory explanation in the assumption of considerable transverse shift-faults.

Manipa Strait — between Ceram and Boeroe — furnishes another example, in which the geological connection is interrupted by a horizontal movement of some few dozen kilometers. The deep basin is the track of this fault.

5. The outer arc is more elevated than the inner arc because it is being forced on to the Australian Continent. Timor, which has met with the greatest resistance is elevated most. The Tanimbar Group shows the same structure and stage of development as Timor in the Plio-Pleistocene, namely: a double geanticline with a trough in between. In consequence of the greater freedom for its horizontal movements, it has not developed so far as its relative.

Molengraaff maintained, that the tertiary mountain building forces were possibly more intensive and superficial than the younger movements, but the apparent difference is principally due to the nature of the exposures which we are now able to study. At the surface, the tectonic activity manifests itself by blockfaulting and horizontal transverse faults accompanying horizontal movements and broad geanticlinal and geosynclinal warping. Of the tertiary structure only the deeper elements are exposed, since the old superficial forms were later destroyed. Of the present tectonic activity we see only the superficial results, whilst the same intensive processes are going on below the surface.

This conception is worked out by Brouwer in some detail. The geanticline is elevated and eroded. The consequence is, that rocks that first suffered the deep seated processes, are now subjected to the superficial movements, where the forces and rate of movement are less. Irregularities in the elevation and erosion and differences in horizontal speed of motion cause the present geanticlines, indicated by the direction of the coast lines, to cut the tertiary strike under oblique angles. Ceram, Timor (and Japan) are cited as examples of this phenomenon.

Another indication of this process is that: „Considerable transverse fractures near the surface of the moving geanticline, coincide with bending points of the horizontal projection of the geanticlinal axis" (bibl. 23, p. 576). Manipa Strait and both ends of the island of Timor are given as examples of this (bibl. 23, p. 575).

Respecting the connections of the arcs, Brouwer offers some fresh suggestions (fig. 39). The Soela Islands, the southern part of Obi and Gomoemoe further to the south and Misool are considered as being the foreland of the northern part of the Banda Arcs, in the same manner as the Sahul Shelf is in respect of the southeastern section. The northern part of Obi belongs to Batjan and Halmahera. The latter island is continued through Morotai and on to the Palao Islands lying far away to the northeast and via Salawati-Waigeo to northern New Guinea. Groot Kai may find its continuation across the Aroe Deep to the southwest of New Guinea, south of the Mac Cluer Gulf, for both regions show strongly folded Tertiary.

In a recent publication (bibl. 25) Brouwer reported the results of his expedition to Celebes. The tertiary strike here too meets the coast at considerable angles. Faults play an important part in the present configuration. The island forms an intermediate stage between the structure of Borneo and the Molukken.

Van Es, when considering the relationship between the structure and occurrence of ores, oil and coal (bibl. 36), constructed a map on which the major tectonic features were shown (see fig. 40). The chief differences from the plans already mentioned are the following.

The southern ranges of Java are continued through the southern parts of Bali, Lombok and Soembawa to Soemba and thence onward through the Outer Banda Arc ending in Boeroe.

The „volcanic line" embraces Goenoeng Api (?) north of Wetar, leaving the latter island to the south and ending at the beginning of the Siboga Ridges.

The northern ranges of Java and Madoera are connected with the southern arm of Celebes. The Makassar Strait is considered a fault, cutting off a geanticline running from Kaap Mangkalihat to the northeastern arm of Celebes, the Soela Islands, Obi, Misool and onward into the Snow Range of New Guinea. It is possible that the Soela Islands are the severed continuation of the Outer Banda Arc (bibl. 39, p. 5). A geanticline coming from Mindanao to the Talaud Islands proceeds by way of Halmahera, Waigeo, northern New Guinea and Japan, to the v. Rees and Gauttier Mountains in New Guinea.

Independently of Molengraaff, *Wanner* concluded from the observations which he was able to make during his Timor expedition, that the structure of this island is characterized by a number of overthrust sheets and an imbricate structure (bibl. 122).

In a later publication, Wanner considered the relations in the northern part of the Outer Banda Arc (bibl. 123). From the strike of the pre-pliocene structure of Boeroe — that is N.W. — and also from the resemblance which the rocks bore to those of Soela Sanana, his conclusion was that the

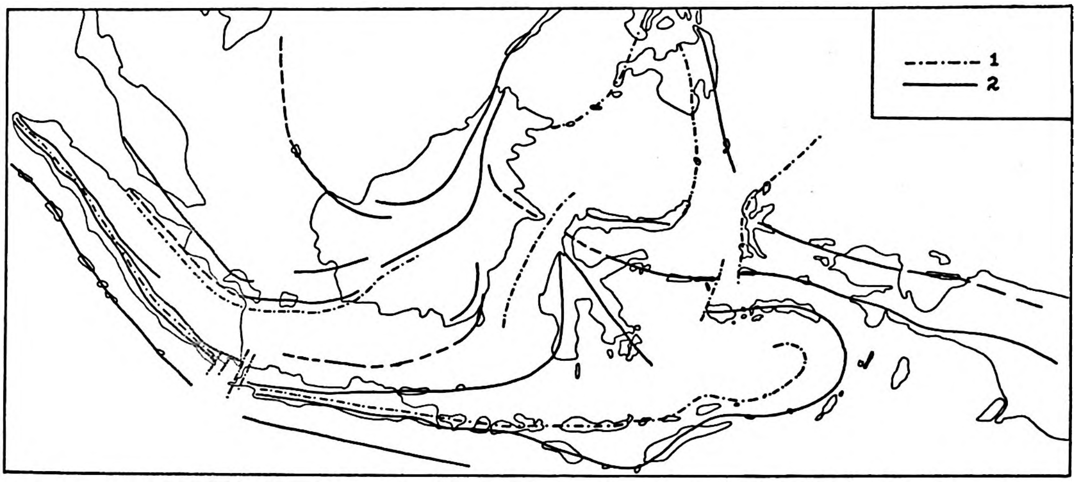


Fig. 40. Geotectonic map of the East Indies after v. Es (bibl. 36). 1 = faults and volcanic lines, 2 = folded ranges.

continuation of the Outer Banda Arc is to be sought towards the Soela Islands, rather than towards the Toekangbesi Islands; as Molengraaff thought possible. Wanner confirms Martin's opinion to the effect that the tectonic pressure came from the northeast (bibl. 78, p. 270) and that Ceram is the continuation in the southwest, but that, here, the force came from the opposite direction. He also confirms Brouwer's theory, viz: that Manipa Strait is a horizontal transverse fault. By transferring Boeroe back to its original position, it would approximately fit on to Soela Sanana, a former connection, which the Sarasins had already maintained on the strength of zoögeographical evidence.

Wanner further presents reasons against considering Misool, Obi and the Soela Islands as forming the foreland of the northern part of the Banda Arc, as suggested by Brouwer. In the first place, the Soela Islands are continued in Boeroe. In the second place, Misool belongs to the same geosyncline as Ceram and cannot be regarded as formed of epicontinental deposits in the way that the Sahul Shelf is the foreland of Timor. Misool and (even the southern part of) Obi show marked dissimilarities and so cannot belong to the same arc as was imagined also by van Es (bibl. 36).

The absence of a continental foreland opposite to Boeroe and Ceram explains why nappes in the Alpine sense were not formed here as in the case of Timor.

Stille (bibl. 107) drew a rather detailed comparison between the deep-sea troughs and the ancient fore-deeps fronting mountain belts. The two have many features in common. If the latter were deprived of their sediments they would be of the same shape and size as the deep-sea troughs. These sediments are a secondary phenomenon caused by exogeneous forces, and their amount is a consequence of the vast area that drains into the basins. Both types of troughs are closely associated

with orogenetic belts. The principal difference between the two is, that, whereas the submarine troughs are fronted by the ocean bottom, the fore deeps are bordered by an ancient foreland. The similarity existing is therefore insufficient for considering the pair as identical phenomena.

According to Stille, the troughs are formed by large scale, secular undations of the earth's crust, which on no account must be identified with the much swifter and more intensive orogenetic movments by which folding and faulting are caused. The elevation of the island arcs is a complimentary happening; also a broad undation, not a horst-faulting. These movements come under the class of epirogenetic movements. These views of Stille are not compatible with Brouwer's theory, viz: the continuous horizontal movements of the geanticlines combined with thrusting in the deeper crust. The thrusting has occurred during orogenetic phases — in Stille's opinion — whilst the formation of the arcs and troughs happened afterwards as the consequence of epirogenetic undations. The two do not take place in the same period, and no horizontal component enters into the present phase.

According to Stille, Brouwer has failed in proving conclusively, that horizontal movements are now in progress. The phenomena that Brouwer pointed out as proving horizontal movement can be explained in other ways. Thus, the Outer Banda Arc follows the irregularities of the edge of the Sahul Shelf, whilst the inner arc has a less complicated, evenly curved shape. According to Brouwer, this is a consequence of the horizontal movement of the outer arc, by which it pressed up against the Australian Continent, and gradually accommodated itself to the forms of the latter. Stille, on the contrary, points out, that the adjustment of the arc to the shape of the continent may be a primary quality. All mountain chains are adapted to the shape of their foreland and there is no reason to give a different explanation of this general phenomenon for the Banda Arc. Possibly the adaptation was heightened during the tertiary orogenetic phase, but not during the present epirogenetic undation.

Adjacent islands in the Outer Banda Arc often show noteworthy differences in their geology (Sermata Islands). Kisar, which forms the natural continuation of Timor, on the other hand, is placed backwards of the general course of the geanticline. According to Brouwer, this is a consequence of recent horizontal movements, whilst Stille holds that these anomalies were brought about during the tertiary orogenetic phase, and by the recent elevation were only brought to light.

The reef caps of certain islands are tilted and Brouwer is of opinion that the morphological geanticline is moving away from underneath them, dropping them down on the rear side, after having elevated them as it came along. This takes place where the geanticline finds the room to advance. In places where the foreland obstructs the horizontal movements, the islands are elevated, without tilting. Stille instances the case of Roti which is fronted by the edge of the Sahul Shelf and cannot therefore have moved forwards as freely as did Jamdena, which lies opposite to a deep embayment of the shelf. Yet the reef cap of Roti is also tilted, as Brouwer himself showed. On the other hand the degree of elevation of this island is far below that of Timor although it is the same short distance from the shelf and should consequently have been obstructed and raised correspondingly.

In a publication which appeared a short time ago *Umbgrove* (bibl. 119) summarizes his former results as to the age and type of diastrophism, for the entire archipelago. The principal finding from our point of view was the following. Clinching proof was obtained that on all islands where overthrust structures have become known, the orogenetic period was the same and, moreover, of comparatively short duration. These islands are all situated on the belt of strong negative anomalies of the force of gravity discovered by Vening Meinesz.

He further concludes, from the nature of the tertiary sediments, that the former extent of shallow and continental regions was much greater, thus confirming Molengraaff's opinion on the formation of the relief since the orogenetic period from a comparatively flat area.

The reader is referred to this important publication for details. In the same volume will be found also Umbgrove's summary of tectonic theories, relating to the East Indies, in addition to the final results and conclusions of Vening Meinesz on his maritime gravity survey of the East Indies. A chapter on the relations existing between morphological forms and gravity was contributed by myself.

There still remain a number of tectonic theories on the formation of the East Indies, that need

not be included in this summary. They are either dealt with, separately, in the following sections, or else the examination of the new chart throws no light on their postulates, so that they will be left out of account.

2. BROUWER'S THEORY ON THE HORIZONTAL MOVEMENTS.

It is not my intention to give a detailed discussion here on the whole problem of horizontal movements. Nearly all geologists assume that in the formation of mountains and island arcs, horizontal movements have occurred to cause the folding or thrusting of the strata. A much smaller amount of movement could account for the elevation of the arcs and the depression of the troughs, if these are the outcome of warping of the earth's crust by compression. Opinion is divided on the problem of whether the two sets of phenomena are the result of the same compression. These horizontal movements will be left out of account here. And neither will the movements implied by continental drift be considered as they are dealt with separately in a later section.

The problem which will be considered, at present, is that of the horizontal movements of the arcs, as advocated by Brouwer. We have seen that Brouwer believes that the deeper laying strata perform relatively great horizontal movements, in which the surface strata do not participate.

The latter perform vertical, up- and down-movements, when the geanticlinal crest passes under them. The evidence in support of this theory has already been given. The chief points are: 1. the accommodation of the Outer Banda Arc, in shape, to the outline of the Australian Continent, 2. the anomalous strike of the strata in the vicinity of bending points in the geanticline, 3. fractures and faults about these points, 4. the peculiar position of Kisar, 5. the anomalous geological composition of adjoining islands in the Sermata Group, 6. the tilted reef caps of islands on portions of the geanticline which are moving horizontally, 7. the greater elevation and the symmetry of the reef caps of the parts of the geanticline, which met with the resistance of the continent first.

Stille, Brouwer's chief opponent, believes that the arcs and troughs are the result of broad warpings of the crust. These undations follow the tectonic period of the Tertiary, but have no appreciable horizontal component of their own. He believes that the points 4 and 5, just mentioned, are the result of differential movements during the tertiary diastrophism, revealed by erosion after elevation. I believe that both explanations are plausible, so that a decision is not possible on these points.

Stille, in discussing point 6, draws attention to the fact that Roti has a tilted reef cap although it is not opposite an indenture of the Sahul Shelf. We might also turn to Salajar, which has a tilted reef cap (Verbeek bibl. 121, p. 31—40, fig. 7), although the position of this island and its relation to Celebes would appear to exclude the possibility of a great horizontal movement of its substratum. The theory advanced by Verbeek, viz: that it is a block-faulted horst is more likely.

On reference to the new chart, it will be noticed that Kisar is the highest part of a series of ridges between the Inner and Outer Banda Arc. It cannot be considered to be a portion of the outer arc that was displaced horizontally. Point 4 therefore also loses strength of proof concerning horizontal movements.

As to point 7 the new chart shows that Timor is further removed from the Australian mass, than either Roti or Jamdena. The depth of the intervening trough is also greater in accordance with its greater breadth. Neither the symmetry of the reef cap, nor the larger amount of elevation of Timor, as compared with the other two, can be explained by its encountering the resistance earlier in a gradual horizontal movement towards the continent. In this respect the new chart is more in accord with Stille's opinion.

It is therefore to the first three points, that we must look for confirmation of the theory of horizontal movements. With reference to adaptation of the outer arc to the Australian Continent, the new chart furnishes interesting evidence. In the first place we note, that the 200 m line does not give a correct picture of the edge of the continent. For the part south of the Aroe Islands the steep edge begins at about 500 m to which depth the shelf slopes down gradually. This deep position of the edge of the continent may be of importance (see page 56), but that does not alter the fact that the Australian block is deliniated by the 500 m line in a manner quite different to what has

been believed up to the present. The interesting point is, that the indenture opposite to the Tanimbar Islands has almost disappeared! On the contrary the eastern edge of the trough is almost straight from the Aroe Islands to opposite Roti. Instead of bulging into an indenture of the Sahul Shelf, Jamdena is closer to the edge of Australia than any other part of the arc. This is in direct contradiction to the conception, that the outward bend of the arc could have been caused by a greater freedom of movement of the bulging portion. If the adaptation of the originally gently and evenly curved arc to the shape of the continent were completed, the distance of all parts to the edge of the continent would be about the same. This would mean the final stage, which could

Fig. 41. Outline of the arcs of the Banda Sea and the Australian Continent. Scale 1 : 10.000.000. (To a certain extent it is a matter of taste how the precise shapes are drawn: compare fig. 47, showing slightly different forms).

have been preceded by stages in which the distance was still greater for the parts which had already commenced to bulge outwards. If the bulge is a consequence of adaptation it can never go so far as to approach the continent closer than the neighbouring portions (fig. 41).

A second point on this matter is remarked when we come to consider the portion of the arc between Babar and Jamdena. With a view to ascertaining whether and how the arc bends outwards, a number of sounding lines were run, so that a fairly clear picture of this region has been obtained. Although Babar and Jamdena hang together, more or less, the shape is not what would be expected, if an originally straight geanticline was bent forward. The island Dai of the Babar Group, namely, is continued in a series of banks behind the Tanimbar Group; possibly it is even Babar itself that belongs to this ridge. Jamdena and Selaroe and also Masela south of Babar are continued in two banks running to the southwest, the latter as far as about 129° E. The whole structure is governed far more by an interchange of two major geanticlines, having minor ridges, than by an outward bending of one single geanticline (see fig. 41 and also Plate I, fig. 6).

An interchange and overlap of two geanticlines are necessarily primary and cannot be brought about by relative differences in horizontal speed of motion.

In this manner the new chart appears to me to demonstrate by two independant features, that the forward position of Jamdena cannot be an effect of adaptation of the arc to the shape of its foreland.

There is another part of the foreland block which does show a deep indenture, namely opposite the Kai Group. What is the position here? At first sight one might be led to the conclusion that the arc here has adapted itself to this space. Closer analysis of the structure, however, throws a different light on this matter.

On the new chart we observe (see also fig. 41) — to the east of Groot Kai — a submerged ridge strongly recalling this island, in shape and direction. If this ridge belongs to a geanticline that continues on to New Guinea, the whole problem would have a different aspect. Pending further soundings we must for the present suppose that this is not the case. But can the forward position of Groot Kai and its submarine neighbour be the result of a bending outward of the geanticline? Manifestly not, for the rear of the geanticline runs in a smooth curve from the Tanimbar Group to the south coast of Ceram. The principal mass of the geanticline swings past the bay in the Australian Continent without even the slightest ruffle, but two or three secondary geanticlines are joined onto its front side, broadening the geanticline out to twice its normal size. This shows that the geanticline has not moved forward into the indenture offered by the opposite side of the trough, but that the extra available space has been filled by geanticlines on the outer flank of the main arc.

Brouwer pointed out that the position of Kisar might find its explanation in the influence of the sharp corner of the Sahul Shelf opposite to it. We have already noted, that Kisar now appears to form part of a set of ridges between the arcs. The sharp corner of the continent, moreover, has nearly disappeared from the new chart.

Finally, we should consider the relation of Timor to the foreland. As this island forms a distinct loop backward we should expect to find a corresponding bulge of the shelf opposite, to account for the lagging behind in the outward movement of the geanticline. The absence of any signs in the shape of the shelf's edge, or of the trough, of a resistant peninsular is unfavourable to the theory of adaptation by differences in horizontal movement.

If the arc had been cast against the mould of the continent, its western continuation, beyond the limit of the Australian mass, would have met with even less resistance than those parts opposite bays on the continent. If, therefore, the arc bulged into these bays, it would also bulge around the margin of the continental block. Such an adaptation is entirely absent (see fig. 41 and 47).

There is yet another point which should be noted. According to Brouwer's theory the two sides of the Timor-Ceram Trough are formed by two entirely different elements. On the one side, we have the surface expression of the thrusting nappe, on the other side the border of the resisting continent. In some places the two elements are wide apart, in others they have already been thrust up against eachother. One would expect to find a marked lack of symmetry in the section of the trough. Reference to the sections 42—46 on Plate VI. shows the trough to be remarkably symmetrical. Only by exaggerating the vertical scale 10 times (fig. 22) does it become apparent, that the slope of the arc is slightly steeper than that of the continent in most, but not even in all sections. Besides, the depth of the trough increases in proportion to its breadth. The shape accords very satisfactorily with the theory of synclinal compression or epirogenetic down warping, but it reveals no indications of formation in the sense of Brouwer. The frequency of far steeper slopes in the East Indies proves the possibility of the formation of sharply inclined slopes. This would lead one to expect a much more precipitous slope on the frontal regions of a thrusting nappe, than is actually found.

Fig. 42 represents a combined section of the two arcs across Timor and Alor with the intervening troughs, with the heights exaggerated by 10. This picture once more brings out the improbability of the conception, that two sets of thrusting nappes underlie the deliniated surface section. On the contrary, it enforces the view that the present relief was produced by movements without a horizontal component of any importance.

To sum up, the new chart has given several convincing indications, that the shape of the Outer Banda Arc cannot be the outcome of relative differences in horizontal movements in an arc's endeavour to adapt itself to the shape of the Australian continental block.

The next point to be considered is that of the anomalous strikes near bending points in the geanticlines. We have already learned, that there is no real bending point of the geanticline between Babar and Jamdena, but an interchange of two geanticlines. Moreover, the strike on Babar points almost exactly towards the principal bank at the back of the arc behind Jamdena. This strike is therefore not evidence of a former relative position of the geanticline to the northeast, now replaced by an outward bend to Jamdena, but of an unaltered continuation of the Babar geanticline along the rear of the second geanticline of Jamdena.

A second instance of anomalous strike in a bending point of the geanticline is given by Brouwer, in Halmahera and Morotai. This instance was based on the belief, that the geanticline is continued from these islands to the east and onward to the Palao Islands. Now that the echo soundings have proved that the Mindanao Trough runs along the east coast of these islands, and that the geanticline continues from Morotai into the Snellius Ridge east of the Talaud Group, the strike of Halmahera and Morotai points quite normally in the direction of the geanticline (see fig. 39).

The sharpest bending point of the geanticline, namely from Boeroe to Soela Sanana, is not accompanied by anomalous strikes, so far as is at present known. On the other hand, the anomalous strikes of eastern Ceram and on Moa are found on relatively straight portions of the geanticline. Only the strikes on Timor could be considered anomalous, but the bending of the geanticline is but very slight here. Molengraaff's explanation by faulting seems as likely.

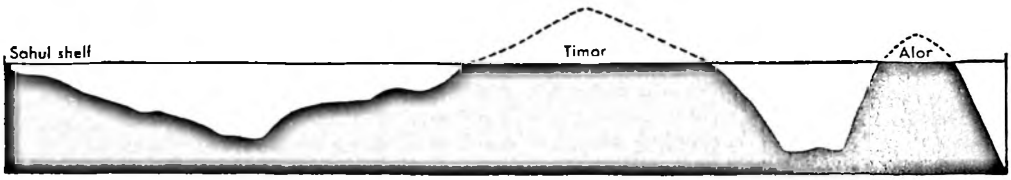


Fig. 42. Section from Sahul Shelf through Timor Trough, Timor, Sawoe Sea and Alor to Banda Basin on the right. Horizontal scale 1: 2.000.000, vertical scale 1 : 200.000.

The last point raised by Brouwer for proving horizontal movements, is that of fractures occurring near bending points in the geanticline. The possible causes of such fractures and faults were treated principally from a theoretical point of view. We have seen elsewhere that what were formerly considered to be submarine faults are, in reality, basins, and that actual faults are found as clean-cut steps on the sea bottom, unaltered by erosion, as in the case of subaerial fault scarps. This lends a somewhat different aspect to the problem of submarine faults.

The Boetoeng Trough has clearly nothing to do with a fault and the Toekangbesi Group was not moved to the northeast (see the report on the coral reefs in the Geological Results of the Snellius Expedition, Volume V, Part 2 by the present author).

The trough in Manipa Strait has a three cornered shape and is closed at the south, by a ridge from Ambalaoe to the south of Ambon. It is not a simple fault, but it might serve to indicate the position of the transversal faults deduced from the geology of the surrounding coasts. Brouwer and Wanner have shown that the rocks and the strike of eastern Boeroe and western Ceram have been relatively displaced. Without a doubt this is a strong argument in favour of horizontal movements, but not necessarily of a difference in larger horizontal movements, as Brouwer thinks possible. The *relative* displacement may be also the *whole* movement. A more detailed knowledge of the geology of Boeroe should be awaited, before a more positive opinion is possible.

Meanwhile, we must note that apart from minor irregularities the steep drop into the Boeroe Basin runs along a straight line right across the northern end of Manipa Strait and that Boeroe and Ceram are also joined by a relatively shallow area, at their northern coasts. To the north and south of the Manipa Basin a connection exists between Boeroe and Ceram. Consequently, it is only the old substratum that indicates a bending point or fracture; the present morphology has a straight course, locally interrupted by a (fault?) trough. This is different to the bending meant by Brouwer. According to his conception, it is the old shape which is straight and the new bending causes the fractures and faults. Here, the deeper stage of tectonic activity possibly resulted in a transverse

fault, the surface stage may be characterized by fault troughs or bucklings, but these do not show any large relative horizontal movement.

As to the structural meaning of the other straits of the Outer Banda Arc, it is difficult to arrive at a definite opinion. Some appear to be the lowest point where two secondary geanticlines interchange: Babar — Jamdena, Timor — Leti, Meaty Miarang — Sermata Group, Sermata Group — Babar.

Especially the latter is very clear (fig. 43). Babar is continued to the west in a ridge, nearly 1000 m higher than the trough dividing it from the Sermata Group, and that continues as far as the passage between Sermata and Kalapa. Masela, to the south of Babar, crowns a separate ridge running in a southwesterly direction, about as far as the northern ridge just mentioned. We have already treated this matter (see page 36—37).

It is not the opinion of the present author, that faults and fractures do not occur. Not only have they been mapped on the islands, but the cutting off of the strike by the coasts, and the rather sudden

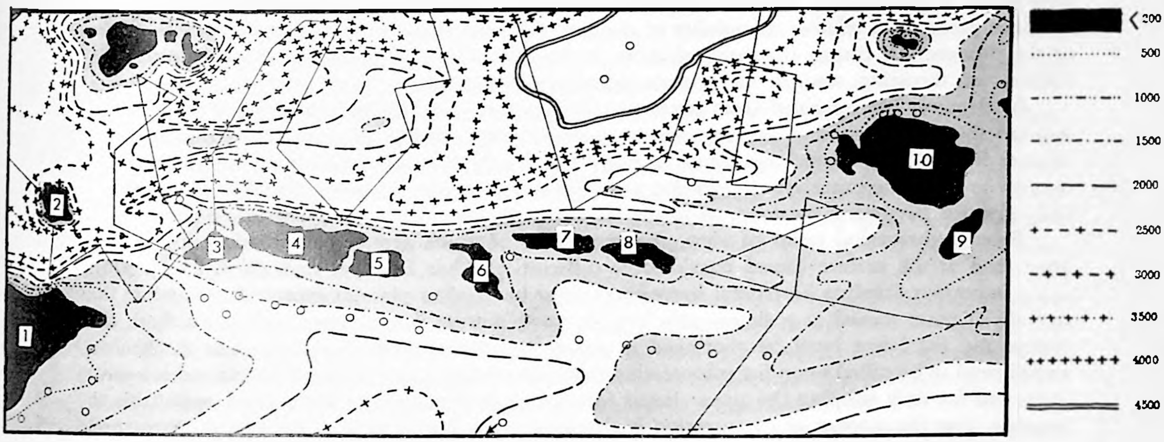


Fig. 43. Bathymetrical chart showing the interchanging relationship between the crests of the secondary geanticlines between Timor and Babar. Scale 1: 2.000.000. Thin lines = echo sounding sections, circles = other soundings. 1 = Timor, 2 = Kisar, 3 = Leti, 4 = Moa, 5 = Lakor, 6 = Meaty Miarang, 7 = Kalapa, 8 = Sermata, 9 = Masela, 10 = Babar.

rise of many islands from the ridges which they crown, also indicate the presence of important faults. It is only believed that these faults are not placed in such a manner as to prove horizontal movements of greater extent than the observed amount. They need not indicate differences in much greater horizontal movements, and many only show a vertical component.

A final point must be raised in relation to horizontal movements, in the sense of Brouwer's theory. The thrusting in lower layers causes the deeper elements of the structure to pass underneath the islands like a wave beneath a sheet of ice. By this mechanism, Brouwer seeks to explain the tilted position of the reef caps. Of the older movements the deeper structure has now been exposed by erosion. Now Umbgrove showed (bibl. 119) that all thrust structures of the Outer Banda Arc are of the same age and were produced during a relatively short period. If we follow Stille, in believing that the periods of orogenesis are short and divided by long periods of rest, then the observed synchronism represents the normal case. With Brouwer's theory of gradual and uninterrupted development, intensive faults or thrust structures have developed in all periods. Synchronism would only be observed if all the exposures should happen to have been formed in positions such as would bring to light the formations of one short section of the prolonged period of thrusting. This would imply not only a stupendous amount of thrusting throughout the whole process, but also a very curious restriction of erosion unaccounted for in the theory. One would at least expert some of the exposures to have laid bare the results of diastrophism of some other period of the long continued orogenetic process.

Moreover, the fact that the thrusting of the miocene orogenetic period is now exposed in the

Outer Banda Arc, also in its most forward positions (Jamdena, Kai Islands), appears to indicate, that the site of diastrophism has not changed drastically in the meantime. The position of the line of negative anomalies coincides with that of the exposed thrust structures and furnishes further proof of the relative immobility of the site of the orogenetic activity.

The same is indicated by the facies of the tertiary rocks. If Brouwer were right, all the tertiary rocks of Jamdena and Kai would have been formed in the broad trough between the arc and the Australian Continent. Only recently were they raised to and above sea level by the advancing swell of the deep lying thrusts. In actual fact there are two transgressions in the Tertiary and many rocks of decidedly shallow facies (bibl. 75, p. 644—645).

Before leaving the subject of large horizontal movements a few general comments must be made. A detailed analysis was attempted of the various points, raised by Brouwer in order to prove the importance of the horizontal movements and especially the differences in horizontal movements between neighbouring parts of the same geanticline, an analysis based on data that in general were unavailable at the time that Brouwer proposed his theory. The result was that all the more important evidence points to a relative immobility of the site of diastrophism and the intermittent character of the process. (Of course the opposite sides of the belt advanced towards eachother during the folding and thrusting, also, in the present author's opinion).

As Brouwer has refrained from expressing his opinion, graphically, in a series of (theoretical) sections showing the gradual development from stage to stage of the „moving geanticlines", in the manner followed by Argand and Staub in respect of the Alps, one is left in doubt as to the exact manner in which he conceives the development. This must excuse the present author, if he has not fully grasped Brouwer's meaning, as to the following matter.

From a theoretical point of view, the conception of much greater compression in a deeper layer than at the surface causes considerable difficulties. Thus Brouwer says (bibl. 23, p. 576): „. . ., important absolute horizontal movements must have taken place at greater depth, while the superficial parts moved at a slower rate." If the earth's crust is compressed, all parts, both the higher and the lower must be shortened in about the same degree. A very curious mechanism would need to be called upon for compressing the deeper strata into overthrust sheets and intensive folds and for only warping the upper layers into broad geanticlines. It seems more reasonable to suppose, that the amount of compression is about the same for all depths, but that the structural forms resulting are dependent on the tectonic level.

Without further proof to the contrary, I am of opinion that this should form the basis of our deductions. The site of these compressions may be slightly different for varying depths, but the great independence of phenomena at the surface and in the depths, which Brouwer assumes, is in my opinion unwarranted by the available data. (The layer of the downward fold of Vening Meinesz is, of course, much deeper than the ones under discussion here).

3. LAWSON'S THEORY ON THE FORMATION OF INSULAR ARCS AND FORE-DEEPS.

In a recent article Lawson proposed a new theory to account for the formation of insular arcs (bibl. 73). The main elements of the theory are the following: Originally the earth's crust consisted of sima only, formed by an upper layer of basalt covering a deeper substratum of dunite. Orogenetic stress produced basaltic continents. During the denudation of these the salic elements were concentrated, the basic parts being carried to the ocean in a state of solution. Isostatic uplift of the denuding continent brought new basalt to be weathered and denuded. The isostatic sinking of the adjoining parts where the salic waste products were deposited, resulted in the fusion of these, p. 359: „The collapse would result in every case in the genesis of a new mountain range with a granite core." In this manner the continents became continually more salic and thicker. The continent is differentiated into granite on diorite. The double layer of sima is continued under the continents.

The arcs are explained as follows: p. 367: „In this hypothesis the arcs are regarded not as uplifted seafloor, but as true continental margin, which has been underthrust by the landward creep of the oceanic sima. The creep is the manifestation of the urge for isostatic equilibrium which is

upset by continental denudation. The form of the festoons arises from the geometric fact that the trace of any large trust-plane slicing the spheroid of the earth is necessarily arcuate. The best illustration of this is the splendid arc presented by the southern front of the Himalaya."

The flow of basalt towards the continent results in the formation of open fissures which are filled by dunite rising from beneath. Isostatic compensation is soon reached, on account of the higher specific gravity of dunite. The resulting hollows are the great foredeeps of the insular arcs. The geosynclinal seas at the back of the continents are foundered areas of the continent from beneath which the basalt flowed under the rising continents and was replaced by dunite. The dunite is heavier and therefore the continent lies deeper here.

If we consider a few major objections which can be raised to this theory, it is not necessary to go into all details.

To my mind the following weak points need consideration.

1) All processes are described as the result of isostatic equilibrium being strictly maintained, yet the primary formation of the basaltic continents occurred without any compensation. They were denuded and therefore rose above sea level, that is, a few thousand meters above the general surface of the primaeval ocean bottom. Nothing compensated this enormous load and yet they are even supposed to have risen isostatically during denudation.

It is not possible to suppose that the primary basaltic layer was thickened locally and floated up by the dunite substratum, because the basalt is assumed to be liquid. This follows from the assumption that depressed masses of the present continents melt and thus produce the granite batholiths. This implies a molten state of the substratum. Moreover, even up to the present period the basalt flows in under the rising continents. The conception of a floating mass is incompatible with a constitution of that mass which allows of easy deformation and still more of flowing movements through hydrostatic pressure.

2) It has been proved that isostatic equilibrium is attained for comparatively small areas. The crust of the earth can sustain the weight of a single mountain, but not of a whole range. In other words, compensation is local for parts of at most a few hundred square kilometers. Lawson's theory, however, implies that compensation is effected for large sections of a continent at a time. Areas of tens of thousands of square kilometers are depressed or uplifted *en bloc*.

3) Many arcs are double, but the theory does not account for this important phenomenon. Neither is an explanation offered for the universal occurrence of volcanoes on the island arcs.

4) The Bonin-Marianne-Pelew arc, as Lawson himself points out, cannot be explained in the same manner, as it is situated far out in the sima of the Pacific. A different explanation is therefore offered, although it resembles the normal arcs in all other respects. This explanation is, moreover, far from satisfactory. The great Brooke Deep is thought to exert a horizontal pressure on account of its greater depth, a result of differential shrinkage of the earth's crust, and consequently greater load of water. Not only is the shrinkage an element which does not fit into the conception of a mobile sima, but the load of 1000 meters of water would have to result in the elevation of 5000 m of sima. As it is sima that is elevated to sea level there should be a local, positive isostatic anomaly of the order of 500 milligals, but Vening Meinesz found only slight deviations from isostasy.

5) The arc is supposed to be the edge of the continent and the trace of a thrust plane. No explanation is offered why the edge of the continent should have this geometrical shape. If the original edge of the continent had an arbitrary shape, the geometrical shape of the supposed flat thrust would cause this plane to cut through the sima here and there. Evidently this has not taken place.

6) The Soenda Arc passes along the edge of the Australian continent. In Lawson's theory it would have to end abruptly where this continent screens off the influence of the ocean bottom.

7) The gravimetrical results of Vening Meinesz in the East Indies can hardly be said to fit in with the fundamental assumptions of the theory.

The explanation of the curvature of the arcs Lawson has borrowed from Ph. Lake. Lake's theory is discussed separately below.

4. LAKE'S THEORY ON THE FORMATION OF ISLAND ARCS.

In 1903 Sollas showed that many mountain- and island arcs are truly circular, when measured on a large-sized globe (bibl. 103). In a recent article Lake followed up this line of speculation (bibl. 71). With the aid of accurate calculations he showed that the Kurile-, Philippine- and Riu Kiu Arcs correspond almost exactly to true circular arcs. The Japanese Arc is not a good circular arc (the same could have been said of the East Indian Arc).

Sollas also showed that the poles of several of these arcs are situated upon the same great circle. This was corroborated and extended by Lake. The great circle through the poles of the Kurile- and Philippine Arcs passes 1°53′ from the Riu Kiu pole and 0°25′ from the East Indian pole. The Japanese Arc has its pole at a distance of 4° 28′ from the great circle. The great circle through the poles of the East Indian- and Himalayan Arcs passes 1° 25′ from the pole of the Iranian Arc (Lake's figure 4).

Lake attempts to explain these geometrical properties of the arcs along the following lines. If the arcs are formed by thrust-planes and these are truly flat planes, they must cut into the sphere of the earth along a true circle. For the Himalayan Arc the dip would have to be 14°. Middlemiss showed that in Jammu Province the dip of the boundary fault below the mountain range is 12° to 15°. Lake concludes on p. 151: „. . . it seems reasonably certain that the Himalayan arc owes its shape to the fact that it is the edge of a mass which has been pushed southwards upon a great thrust-plane dipping northwards at an angle of about 14°". Of the island arcs he says: „. . . it is reasonable to suppose that, like the Himalayas, they owe their shape to the fact that they rest upon thrust-planes." These planes are at right angles to the plane of the great circle passing through their poles. Lake suggests that the Asiatic Continent is being underthrust from the East by the Pacific Ocean floor and from the south-west by the Indian Ocean floor, the movement being at right angles to the plane of the great circles. Movement of the continent over these thrusts is possible without any breaking up of the moving mass.

The problem presented by Sollas and Lake of the notable accuracy with which many arcs of the world follow circles cannot be lightly dismissed. Most tectonic theories have either ignored the problem altogether or only given it slight attention. But if no fundamental principle had regulated the development of the ranges of arcuate form, irregularly curved chains would have resulted, and there must be some reason why the circular shape is preferred to the other simple geometrical curves. It is therefore of great importance that Lake has given a simple explanation. The flat, dipping thrust-plane he assumes is no doubt far more probable than that a conical or cylindrical structure in the earth has played a part. On the other hand it cannot be denied that there are some objections to Lake's explanation.

In the first place, some arcs, like the East Indian arc, deviate considerably from the circular shape, and the ends may deviate from it entirely. Other chains or arcs bear no connection whatever to circular shapes, either for a smaller or for a larger part of their length. Lake's thrust-plane is so fundamental a feature of the mountain formation that if it really exists we would expect to find all similarly formed mountain chains to show at least some resemblance to the circular shape.

In the second place, all thrust-planes we know are irregular, reacting to local structural elements of the crust.

Lake's thrust-planes must be absolutely flat for thousands of kilometers and must therefore be of a different nature altogether. Lake's thrusts and tectonic thrusts are as different from each-other as stratification is from the layers of discontinuity in the earth's crust. They may of course develop into a normal thrust-plane at the surface, as in the case of the Himalayas, in the same way as for instance a river may change to a tidal estuary near its mouth.

In the third place, the thrust-planes would cut through the earth's crust down to a depth of a few hundred kilometers. Thus, the Soenda Arc must lie on a plane that passes below Borneo at a depth of about 300 km. A movement along this plane would upset the structure of the earth's crust and is quite incompatible with the theories of the existence of sial and sima, and difficult to reconcile with isostatic equilibrium, and the gravity field of the East Indies.

In the fourth place, there seems no reasonable explanation why the elements of the earth's crust which themselves follow the curvature of the globe should produce a thrust-plane that is entirely flat.

In the fifth place the very considerable and variable axial pitch of the structural elements of the Alps (Argand) and the Moluccas (Molengraaff) is difficult to reconcile with a perfectly flat thrust at the base of the structure. Likewise, the reappearance of portions of the foreland in the central massifs of the Alps argues against Lake's theory.

Although I agree with Lake that the circular shape of many arcs calls for an explanation, I cannot consider his solution satisfactory.

5. RUUD'S THEORY ON THE FORMATION OF ISLAND ARCS.

I. Ruud developed a theory on the formation of the East-Asiatic mountain arcs (bibl. 94). He believes that thermal shrinkage of the lower layers of the earth's crust produces great rifts. These layers then contract and exert a drag on the surface layers, causing them to be compressed into folded arcuate structures. The deep basins on the concave side of the arc are situated where the surface layers were torn apart above the rift in the lower crust.

The theory cannot be applied to the Soenda arcs, as the western sector shows the deep depression on the convex side. Neither is it in accordance with what is now known of the field of gravity in these parts. It is therefore not necessary to enter into the many other objections against this theory, which arise when we attempt to explain the continental orogenetic systems which so strongly resemble the island arcs in structure, the geosynclinal period of mountain chains, etc.

6. HOBBS' THEORY ON THE FORMATION OF ISLAND ARCS.

Hobbs presented a theory on the formation of island arcs in general (bibl. 55), in which the East Indies were also considered. He believes the ocean bottom presses against the continental margin and thus produces the arc. An experiment to illustrate his views is as follows (bibl. 55, p. 261): to paste a sheet of letter paper upon a board over a circular area and to press the paper with the fingers towards the pasted area from one side. The pasted area represents the rigid shield upon the continent. The more the pressure from the two hands is continued the more the curvature of the forming arc is increased.

As the experiment would give other results if the shape of the pasted area and position of the hands were altered, the theory can only hold if a rounded protuberance of the continent is assumed, against which the arc is bent progressively. This was overlooked by Hobbs and these hypothetical peninsulars are therefore not explained by him. If they exist they must lie deeply buried below the sea on the inner side of the arc.

Another aspect of the theory is that the arcs „pass through a progressive series of changes marked by ever increasing curvature, and the stage of this evolution may be conveniently designated in terms of the angles of the arc which are subtended." (bibl. 55, p. 259).

Hobbs demonstrates that the various arcs show different properties according to the amount of curvature. There is, however, no proof that one arc passes through the various stages or that the curvature has increased during the development. It is just as probable that some arcs have a small and others a larger degree of curvature to start with and that they show different phenomena according to the amount of curvature. Thus the Banda Arc shows a curvature of 200°, the Soenda Arc of only 100°. According to the theory of Hobbs, the former should therefore be the older. From a geological point of view, however, the arcs form one single unit and the age of its parts is the same, as far as we are able to judge. No proof from geological data is given that the arc of smaller curvature in the East Indies is younger.

Another weak point in the theory is that the East Indian arc is subdivided into three separate units, the Andaman-Nicobar Arc, the Soenda Arc and the Molukken Arc. This subdivision is necessary to the theory, for otherwise the whole arc would curve back upon itself. It would also show a varying length of radius which would not agree with the theory. As stated above there is no geological foundation for this subdivision. The break in the curvature of the arc in Soenda Strait is ignored, and also the continuation of the volcanic arc beyond Wetar.

Hobbs maintains that the middle section of the arcs is more unstable than the remainder. Even if his subdivision is taken into account the assumption does not hold for the East Indian arcs. Java, the middle of the Soenda Arc, is not less stable than Sumatra; the Kai Islands, the middle of the Molukken Arc are not more mobile than Ceram or the Sermata Islands; nor are these parts more seismic than the wings of the arcs. In a later publication (bibl. 56) Hobbs seeks to explain the elevation of island arcs by the pressure caused by the progressive settlement of the ocean floor. As extensive parts of the East Indian arcs are not bounded by an ocean floor, this explanation cannot be applied to these, and that being so it appears improbable in respect of other examples.

In conclusion we can say that Hobbs has reviewed all island arcs from one point of view and has pointed out many interesting laws of their structure, but that his conclusions as to the reasons of these differences are open to serious doubts, as far as the East Indies are concerned.

7. STAUB'S THEORY ON THE FORMATION OF THE EAST INDIAN STRUCTURE.

R. Staub has considered the East Indian Archipelago in his synthetical study of the features of

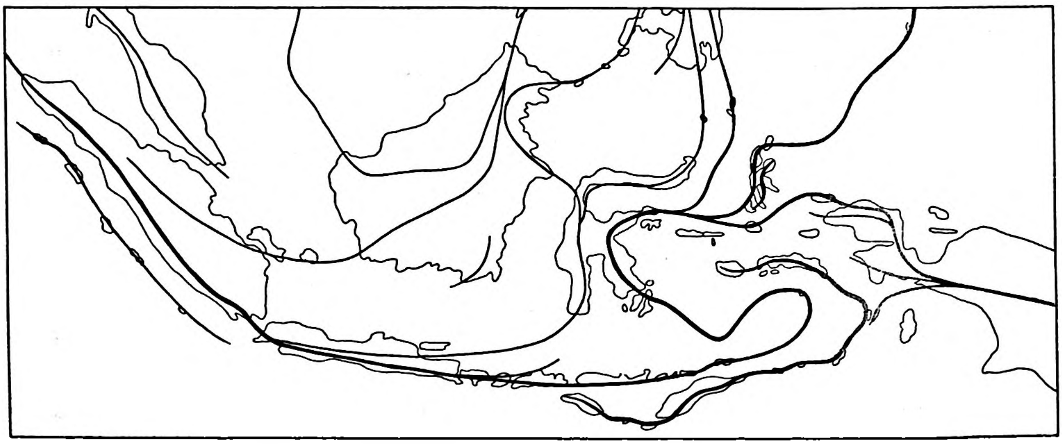


Fig. 44. Structural map of the East Indies, after Staub, bibl. 105, fig. 21a, p. 71.

the earth's structure (bibl. 105, p. 69—77 and 84—92). It is chiefly the plan of the chains and arcs that he attempts to unravel and explain.

Staub believes in the drift of the continents, by which the mobile portions are folded into mountain chains. As a very important element he considers the old, solid parts which belong to former periods of diastrophism, and that offer great resistance to tectonic forces. Many of these hard blocks are incorporated in the younger mountain ranges which curve round their edges, to meet again at the other side. They are represented by sea basins or low country, often covered with young sediments, for example, the Adriatic Sea, the Hungarian Basin. Thus he says (p. 84): „Die Zwischengebirge aber beherrschen als steife Platten den ganzen Stil, und im besonderen auch die Form des Orogens. Halten wir uns dies vor Augen, so werden wir begreifen, dasz die Linienführung des alpinen Orogens gerade hier (East Indies) gar keine einfache sein kann. Da erlischt eine Kette zwischen zwei Zwischenmassiven, dort taucht eine neue in ähnlicher Lage, aber nicht in absoluter genauer Fortsetzung der ersten verschwundenen wieder auf, und solch ein Stil kann sich mit diesem Mechanismus zu Dutzenden von Malen wiederholen."

The Java-, Sulu-, Celebes-, Flores- and Banda Seas are old intra-mountain basins of this kind. The mountain arcs curve up north through the Philippines and away south round Australia, because the old Pacific mass offered an enormous resistance. His map, fig. 21*a*, shows how he believes the connections of the chains should be drawn (fig. 44). They differ somewhat from those proposed

by Molengraaff or Brouwer, without, however, reasons for these alterations being given. They cannot be considered as improvements, but as Staub himself has already pointed out, the exact position is immaterial to the principle involved. I will therefore not enter here into a discussion of his reconstruction. A few more general remarks will be given presently.

Staub considers the very interesting parallel between the Alps and the East Indies, that is, the western and eastern end of the Alpine belt, which had already been presented by Argand (see his figures 22 and 23 and also 24 and our fig. 26). Like Argand, he orientates the two regions in opposite directions and therefore represents the east end of the alpine system with the north at the bottom of the figure. In this way the Alpine-Carpathian Arc is the equivalent of the arc from Burma to the Banda Arc, the Pyrenees find a parallel in the mountains of New Guinea. The Soenda Arcs, it is true, are on a grander scale, but the resemblance is nevertheless striking (see our paragraph on page 59).

An important part of Staub's theory is the disappearance of portions of the foreland below the ranges of the central chain, where it bends outwards. On p. 88 he says: „Vorderindien entspricht dabei dem russischen, Australien dem spanischen Vorlandsegment Europas, und Neu-Guinea beherbergt auf diese Weise die australischen Pyrenäen, und der Himalaya erscheint als ein indischer Kaukasus." On p. 89: „Die tektonischen Elemente des Himalaya setzen somit ohne jeden Zweifel in Neu-Guinea wieder ein." The Outer Banda Arc is compared with the Jura Mountains. The Kai Islands are connected with New Guinea, but Staub points out that here too the intra-mountain masses obscure the direct continuations to a certain extent. Halmahera, Waigeo and surroundings are regarded as deformed parts of the Pacific block.

It falls outside the scope of this investigation to consider the structure of the Alpine chains over the entire globe and we must therefore refrain from discussing the major points of Staub's theory. Some local details which are, however, of importance to the whole theory will be considered.

In the first place, the so-called disappearance of the foreland below the outward bend of the East Indian girdle appears to be founded on insufficient data. In his fig. 21*a* (= our fig. 44) Staub leaves an open space between the insular arc west of Sumatra and the outer Banda Arc eastwards from Soemba. On the chart we find a quite different situation indicated. The geanticlines of the islands west of Sumatra and the Outer Banda Arc are connected right along the outer edge of the orogenetic belt. The new bathymetrical data only emphasize this feature. Beyond this arc there are two other concentric zones. First, the Java Trough which is connected with the Timor Trough and the deep facing the west coast of Sumatra. On the outside a less marked and partly disconnected zone of elevations, two of which are Christmas Island and Corona reef, follows the same convex line. The relation of the latter to Australia has not yet been ascertained. Although the level of these features may be lower in front of Java, they can not in any case be said to disappear below the orgenetic belt, *in fact there is no major element of the foreland which is visibly overridden by the East Indian chains.* Staub's theory that the southward bend of the Alpine elements represents an actual horizontal movement and not a primary loop which already had its present shape at the time of formation, may be correct, but it has still to be proved. This mechanism may be considered probable for the small outward bends of the superficial Jura Mountains, with its many transverse shift-faults. The enormous curvatures of the Alpine chains would imply considerable lengthening of the arc, with open fissures or oblique shift-faults. In the absence of these, the primary outward bending of the arcs seems far more probable. Naturally the structure of the East Indies implies a certain amount of horizontal movement to explain the folds and thrusts observed. Possibly, movements of dozens of kilometers occurred, but to produce the outward loop from an originally straight line, movements would be required of hundreds to thousands of kilometers.

In the second place, Staub explains the arcuate shape of the Alpine chain in Europe by comparing it to straight waves entering the mouth of a bay and bending out in a fan shape in the bay itself (bibl. 104, p. 253 and fig. 70, bibl. 105, p. 49—52). On p. 51, bibl. 105 he says: „Die alpinen Ketten drängen unter dem Drucke des von Süden anrückenden afrikanischen Blockes überall scharf gegen Norden vor. Die geosynklinalen Zentralketten flieszen dabei deutlich in die grosse mitteleuropäische Bucht zwischen spanischem und russischem Vorgebirge hinein." The same explanation is given for the arcuate shape of the East Indian chains. Bibl. 105, p. 86: „Damit verschwinden naturgemäsz auch die himalayanischen Rücklandfalten Gondwanas unter dem vordringenden burmanisch-malayischen Bogen. Derselbe quillt in plastischer

Masse weit über den Aequator vor, das niedergesunkene Gondwana samt seinen Randketten tief unter sich begrabend."

This resemblance in shape of two phenomena (waves and folds) which are of an entirely different nature cannot be used, however, to explain the one by the other. In the case of the waves it is an undulation which is propagated, while no new matter enters into the bay. In the case of the mountain arcs it is the matter which according to Staub moves inwards into the bay, but the anticlines are not propagated with respect to the medium in which they were formed.

The fan-like shape of the waves is due to the physical rules governing the propagation of undulations. These rules cannot be held responsible for the arcuate shape of mountain chains, as these are not undulatory in nature. Moreover, the actual propagation even of the medium is open to grave doubt in the case of the East Indies, for as I pointed out above, the outward bend is in all likelihood almost entirely primary.

Argand (bibl. 3) showed that the structure of the Alps is governed to a large extent by the relative resistance offered by the various parts of the foreland and also that this structure may be explained by a northward movement of the African mass. The plastic nature of the material resulted in diverging movements, according to the shape of the foreland, yet the rigidity was sufficient to render the oblique movements in the western section of the Alps less intensive than those in the central and eastern Alps, which took place in the direction of the primary movement. This does not imply, however, that the geosyncline was originally straight. It is a long cry from this well-founded conception to Staub's hypothesis in which the entire curvature is secondary and the radius of the arc represents the minimum amount of the horizontal movement.

Argand has already used the comparison with waves, but only to illustrate, not to explain the shape.

Staub's explanation of the arcuate shape of the chains is unsatisfactory, as it is based on the false comparison of undulatory waves with the bodily movement of anticlines and the unproved assumption that the chains were formerly considerably less arcuate.

In the third place, the major tectonic line that Staub draws in the East Indies follows along the concave side of the line of gravimetrical anomalies from Sumatra as far as Banda, but deviates from it where it is drawn to Celebes and then crosses over to Halmahera and the Palao Islands. A better agreement would be obtained by connecting Banda with Boeroe and thence on to the Talaud Islands and Mindanao. This would not greatly alter the principles of Staub's theory and would at least fit in better with the new bathymetrical data.

It is a different matter altogether whether the theory can be made to agree with the existence of the line of gravity anomalies. By the line of negative anomalies one arc (that is: the major line of Staub) is singled out from the other secondary lines in Staub's conception with such startling emphasis that we must believe that the nature of this zone is different to that of the others. In Staub's theory all the lines represent similar zones between ancient blocks. The gravimetrical results of Vening Meinesz prove, however, that there must be a fundamental difference, greater than can be accounted for by a mere difference in degree, as would follow from Staub's theory. Umbgrove's geological synthesis also gives irrefutable proof of the same fundamental difference (bibl. 119).

Finally, it should be pointed out that the Timor-Boeroe Arc is a far more important element than the Jura, in fact it is the main tectonic belt in the eastern part. The Inner Banda Arc appears to be of minor tectonic importance and its connection with Celebes is only brought about by a doubtful virgation. These are the chief aspects in which Staub's structural map needs revision.

If we sum up the whole matter, we arrive at the following conclusion with regard to Staub's views. Staub has verified Argand's opinion that the horizontal projection of the East Indian chains bears a marked resemblance to that of the Alps and its satellites. He has failed to prove that the outward bending arc has overridden the foreland, and that the arcuate shape is not primary, so that his explanation of the arcuate shape is not satisfactory. His structural map is not an improvement on former suggestions. His conception of intra-mountain blocks „Zwischengebirge" between folded zones is a good working hypothesis, but it is not confirmed by the gravimetrical data and structure as far as they are yet known.

8. KOBER'S STRUCTURAL MAP OF THE EAST INDIAN SYSTEM.

Kober attempted to find again in the East Indies the more or less symmetrically built orogenetic plan he constructed for the other young mountain chains (bibl. 62). As it lacks a detailed argumentation his rather simple structural map (fig. 6, p. 34, bibl. 62) of the East Indies is not convincing. If we take into account that our knowledge of the structure of this region is still very incomplete in comparison with its complexity, we cannot expect a definite answer to the question of the bilateral symmetry of tectonic build. So far, however, evidence is strongly in favour of an outspoken asymmetrical structure (see also Umbgrove, bibl. 119).

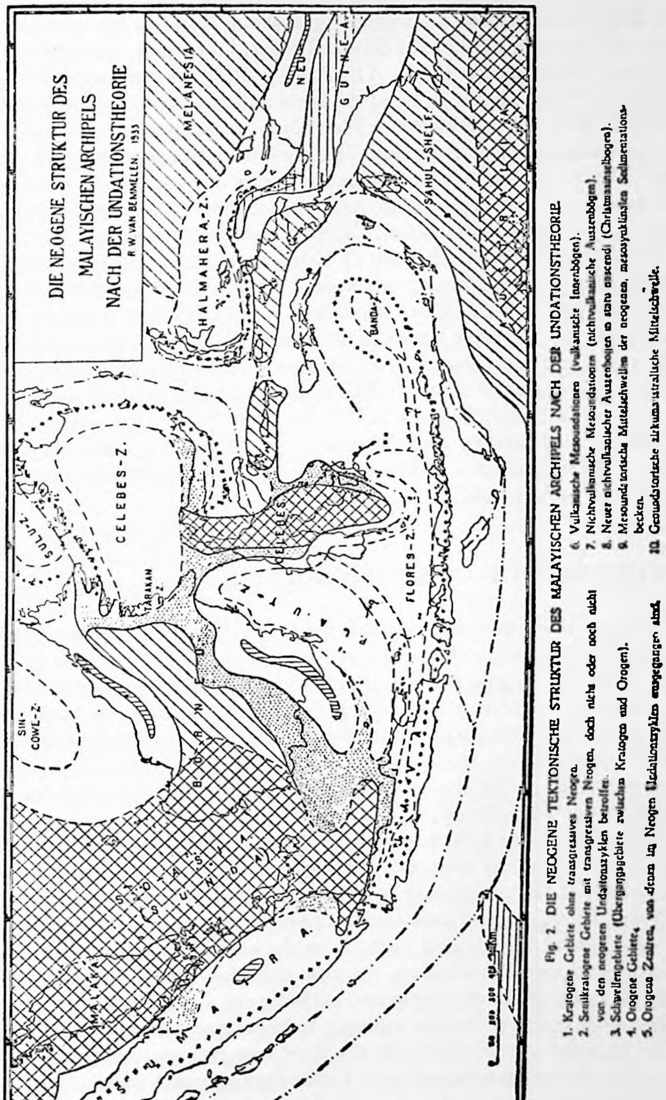


Fig. 45. v. Bemmelen's geotectonic map of the East Indies (bibl. 14, fig. 2).

9. v. BEMMELEN'S UNDATION THEORY.

Some general theoretical objections to the undation theory (see bibl. v. Bemmelen) were brought forward some years ago by the present author (bibl. 66). As v. Bemmelen (bibl. 13) answered only a few of the points raised, that side of the problem will not be entered into again. In an earlier part of this work some observations were made on sliding hypotheses in general (p. 72).

In a recent paper, v. Bemmelen drew a geotectonic map of the East Indies in connection with his theory. Here, some divergencies between this map and the new chart will be shortly outlined without any attempt being made at answering the question of whether the necessary adjustments would affect the principles of the theory.

As various of the tectonic elements of v. Bemmelen's map are supposed to be actual surface features, there should be a fairly strict accord between his map and the chart. For the old chart this

was still the case more or less, but there are important divergencies with relation to the new data. The following are the main points that are in need of adjustment.

The P. Laut orogenetic centre occupies part of the Soenda Shelf and part of the basin of the Makassar Strait and shows no relationship to the morphology (see below, Halmahera centre). The eastern end of the Flores orogenetic centre embraces part of what appears to belong to the Banda Basin. There is no reason for supposing that the banks to the south of the Toekangbesi Group are of a volcanic nature, neither the Siboga Ridges. Goenoeng Api north of Wetar rises straight up from the bottom of the Banda Basin. The Ambon Ridge is connected with both Banda and Ambalaoe. The Siboga Ridges appear to be of the same nature as the Luymes Ridges. The connections between the Aroe Islands and northwestern New Guinea, also between Banggai and southeastern Celebes, are drawn across deep basins obliquely. The regions to the west and to the east of Obi resemble eachother closely. The Halmahera orogenetic centre comprises part of the Mindanao Trough, the basinnorth of Waigeo, the Halmahera basins, part of Halmahera and shows no relation to the morphology (in contrast to the Sin-Cowe-, Sulu-, Celebes- and Banda centres). The Togian Islands and Oena Oena are cut off from the northern arm of Celebes by an important trough. Morotai is continued north in the Snellius Ridge. Finally, one should mark that the subdivision of the basins into moderately distinct groups, which is given elsewhere in this volume, finds no expression in v. Bemmelen's construction. For the three basins of the second group one finds that the Gulf of Bone is „kratogene", the Gulf of Tomini is crossed by a „volcanic meso-undation" and a „kratogene area", the Makassar Strait is an „orogenetic centre." The same heterogenety of the basins belonging to the first group is revealed. The Sulu-, Celebes- and S. E. Banda Basins are „orogenetic centres", whilst the N.W. Banda Basin is apparently considered to be of a different nature. The foregoing comparison teaches that, even if one wished to follow v. Bemmelen's theory, in principle, considerable alterations in his tectonic map would have to be made.

10. THEORIES OF CONTINENTAL DRIFT.

Wegener has touched upon the structure of the East Indian Archipelago in two paragraphs of his famous synthesis of continental drift (bibl. 127). From the rather cursory treatment of this aspect of his theory one might suppose that Wegener considered it to be of minor importance to the problem as a whole, a mere detail that could safely be left for future investigation. (In the last edition of his book Wegener quoted Smit Sibinga's work on the East Indies). In the opinion of the present author the East Indies offer a unique opportunity of putting to a severe test one of the fundamental elements of the theory of continental drift and therefore merit the full attention of those who believe in this theory.

According to the drift theory there are three different cases of interrelation between continental blocks. In the first they drift apart like America from Europe and Africa. On the whole the evidence tends to favour the probability of this occurrence, although opinion is still far from unanimous in its favour. In the second case two continental masses may be forced together, without a primary deep-sea ever having divided them, as in the case of India and Asia, or Africa and Europe. With a few exceptions geologists accept the view that the Alpine chains furnish proof that the continental blocks at both sides have approached eachother to a considerable extent. Whether this phenomenon is sufficiently deep, or shallow enough, to be termed continental drift is still an open question, as is also the amount of movement. In the third case two continents are originally separated by a broad belt of deep-sea: of uncovered sima. Later, they approach one another and are more or less squeezed together. An example of this case are Asia and Australia, which were originally separated by 3 to 4000 km of deep-sea and have drifted together, separating the island chains of the East Indies and Bismarck Archipelago, bending the former round and pressing them into arcuate curves.

Fig. 25 (p. 92) of Wegener's book represents a depth chart of the surroundings of New Guinea. It is a pity that this chart differs in a few scarcely noticeable details from the original, so as slightly to exaggerate the features in favour of Wegener's theory. For instance, the Inner Banda Arc is represented as connecting Banda with the Siboga Ridge and Goenoeng Api, north of Wetar. As a matter of fact this connection was interrupted on both sides of the Siboga Ridge on the above-mentioned

original. Then the depth of the trough between the Sahul Shelf and the Tanimbar Islands is given sa more than 1000 m deep instead of less; in this way the dividing line between Australia and Asia is too strongly marked. Finally, the unfavourable connection between the Soela Islands and Obi is reduced in importance and the connection between Halmahera and New Guinea emphasized. Although subsequent investigation has born out all but the first and last alterations, such instances of a too zealous search for proofs do not increase one's confidence in the author as guide on those aspects of the theory which cannot be checked personally.

Now that we can consult a much improved chart we must attempt to review the theory of continental drift where it deals with the relations between Australia and the island arcs of the East Indies. We must seek an answer to the question whether the continent can be assumed to have approached from a great distance and in what measure and in what manner it may have influenced the structure of the archipelago by its movements.

It need hardly be pointed out that whether this investigation turns out to be in favour of the theory of continental drift or not, cannot be considered as the test „par excellence" of the theory. As Wegener himself pointed out, too many considerations are involved for any single test to be final. On the other hand if, as the present author believes, the available data tend to prove that Australia and New Guinea were always in about the same position in relation to the Archipelago as they are now and that no line can be found along which they came together, a very serious objection to the theory arises. A possible compromise will presently be suggested in case the movements of Australia should be proved beyond doubt from some other source of information.

A) The structure of the Molukken according to the theories of drift.

Wegener himself offered the following hypothesis for the development of the Molukken. During the Mesozoic the westward drift of Asia caused island chains to be pealed off the southeast extremity of the continent. A straight double row of islands ran from Java eastwards to the Bismarck Archipelago. During the Pleistocene the Australian Continent, with New Guinea attached, broke into this chain, bending round the island arcs of the Banda Sea into their present shape. His fig. 4—5 and 24 show the major features of this occurrence, fig. 25 the present configuration (In his fig. 55 the west end of the East Indies is represented, but will not be considered further by us). No further details are given and the origin of the other arcs and troughs is not discussed. It would appear that Halmahera and Obi are considered to belong to the New Guinea block, from the manner in which the depth curves are drawn, but on the world maps these islands are not drawn.

Wing Easton (bibl. 129) gave an entirely different aspect to the matter by assuming that all islands of the East Indian Archipelago were formerly loose fragments which wandered from Australia to Asia and were afterwards caught up by Australia and pressed together. Wegener himself did not consider these alterations a successful attempt (p. 225). On the other hand, he quoted Smit Sibinga's views extensively in the last edition of his book and evidently considered them of great importance.

Smit Sibinga (bibl. 101) worked out a more complicated treatment of the Molukken along the following lines. Before the arrival of Australia a normal double island arc of simple shape was drawn off Asia by the westward drift. The inner arc was built out of the Inner Banda Arc over Goenoeng Api to the Tijger Islands and thence through West-Celebes to the northern arm, the Sangihe Islands to Mindanao. The outer arc contained the Outer Banda Arc to Boeroe, the Soela Islands and Banggai Archipelago, then the Toekangbesi Islands, Boetoeng, the southeast arm of Celebes to the northeast arm, from there via Obi and Halmahera to the Talaud Islands and finally also to Mindanao. He also assumes a temporary connection between Madoera and southwest Celebes. Stratigraphical, petrographical, zoogeographical and structural details are given to show the reasons for this reconstruction. The pressure of the Australian continent buckled the island arcs into their present form, opening fissures between some islands and pressing the end of the arc, broken between the Toekangbesi Islands and Banggai Archipelago, up against the northeast arm of Celebes.

Recently Smit Sibinga had occasion to alter his views to a certain extent (bibl. 102). He now adds a third arc from the Palao Group to Halmahera, Obi and the Soela Islands. The Outer Banda Arc now runs from Boeroe directly to the Toekangbesi Islands.

Smit Sibinga's reconstruction has the advantage over Wegener's first conception (and most other structural considerations of these parts) that it attempts to explain the development of the

entire Archipelago from one point of view, while the latter only deals with the Banda Arcs and leaves the formation of other arcs of quite as intricate a plan to other processes. This is comprehensible, for Wegener only tried to follow the final movements of Australia and had to leave details of the East Indian structure to specialists. That this weak point in the drift theory has not been sufficiently strengthened by Smit Sibinga's synthesis will, I think, become evident from the following discussion.

Argand in his study of the structure of Asia strongly advocates drift (bibl. 4). For the island arcs he adopts Wegener's explanation. He emphasized the importance of compression in a north-to-south direction of Asia, by which the arcs are buckled outwards (p. 296, 321—324).

B) Objections to the drift theory as applied to the East Indies.

As it is only of historical importance to distinguish between the observations to be made on the strength of the new data and those that could have been derived from older information, I will use both together in the following discussion. Neither will an attempt be made to deal separately with Wegener's and Smit Sibinga's hypotheses, as many points apply to both. The following questions will be considered:

1. Does the drift theory in any of the proposed forms give a more satisfactory explanation of the structure of the East Indies than the more conservative theories?
2. Can a morphological dividing line be found between Australia and Asia?
3. Is Wegener's date for the joining of the continents sufficiently well founded?
4. Can a geological dividing line be drawn?

1. *Does the drift theory in any of the proposed forms give a more satisfactory explanation of the structure of the East Indies than the more conservative theories?*

The essential difference between the two conceptions is that the drift theory regards the island arcs as strings of sial-bergs floating in mobile sima, whereas the older conception is of a mobile section of the earth's crust that is being compressed into geanticlines and geosynclines.

Wegener's own suggestion explains the warping of the Banda arcs as the result of pressure by the Australian continent. In itself this is a good working hypothesis, but in this way a separate explanation is offered for one single case of a feature common to all island arcs, an explanation that is not applicable to those parallel cases (West Indies, Northern arm of Celebes, etc.). This explanation therefore brings us no nearer to the solution of the problem of the structure of the East Indies as a whole. On the contrary it makes it more difficult to find a general explanation.

Smit Sibinga's more complicated reconstruction has the advantage of taking a much larger part of the archipelago into consideration. A comparison between Smit Sibinga's map (fig. 46) and the new chart (see also fig. 47) shows, however, that the present relief is in contradiction with many of the submarine connections or ruptures, as presented by that author. The principal points are the following. The submarine ridge, south of Java, is connected with Sawoe and is not broken off the end of Soemba. Kisar belongs to a separate ridge, not to the Outer Banda Arc. The region between Timor and Jamdena is not characterized by fractures, but by interchange of crests.

Goenoeng Api (Wetar) does not form part of the Siboga Ridges. The ridges from Salajar and the Tijger Islands hang together, more or less, with the Inner Banda Arc on Flores. The Boetoeng Trough does not form a horizontal transverse fault between Boetoeng and the Toekangbesi Islands.

The other end of the latter group is supposed at one time to have hung together with northern Boeroe. The present morphology shows a number of intervening ridges. Obi is connected on the west with the eastern end of the Soela Islands, on the east with Misool. The Talaud Ridge is probably continued directly into the northeastern arm of Celebes, and Morotai hangs together with the Snellius Ridge running up as far north as the Nenoesa Islands. It is not unlikely that a deep broad basin cuts across the supposed connection between the Asia Islands and the Palao Group. A number of submarine ridges occur which do not find a place in the proposed system. Besides the Luymes Ridges and the Kisar Ridge, we note the Salajar Ridge, the ridge in the centre of the Ceram Trough, the ridge east of the Kai Islands, the ridge between Ambalaoe and Ambon and, finally, the ridges connecting the Philippines with Borneo.

The foregoing shows that most of the submarine connections and ruptures are definitely in

contrast to the structural connections and ruptures assumed by Smit Sibinga and that a number of elevations exist for which there is no place allotted in the theory. The proposed reconstruction is only possible by our taking into account the emerged portions of the relief and the submerged parts which were already known previous to the new soundings. One is bound to admit, however, that there is no reason to suppose that there is any difference in general importance or structural build between the emerged parts or submerged parts of the same geanticline or between the elevations that happened to have been discovered before, and those found during the echo sounding survey. Thus, we cannot account for the divergencies by assuming that the present connections were brought about after the disturbance in the double arc: if we add Morotai to the ,,Molukken orogen", the Snellius Ridge forms a part as well. If the ridge between Talaud and Tifore is a portion, then the southern continuation is another. If the Siboga Ridge is primary, then the similar Luymes Ridges are also. If the Kai Islands denote a fraction of a separate orogen (the Palao Arc.), the ridge beside it denotes yet another. If the ridge south of Java belongs to the ,,Molukken orogen" as far east as the western point of Soemba, then the continuation, subsequently discovered, cannot be excluded on reasonable grounds. Then there is no space left for fitting in Soemba and it must be added to the ridges which cannot be fitted into a system of two (or three) orogens.

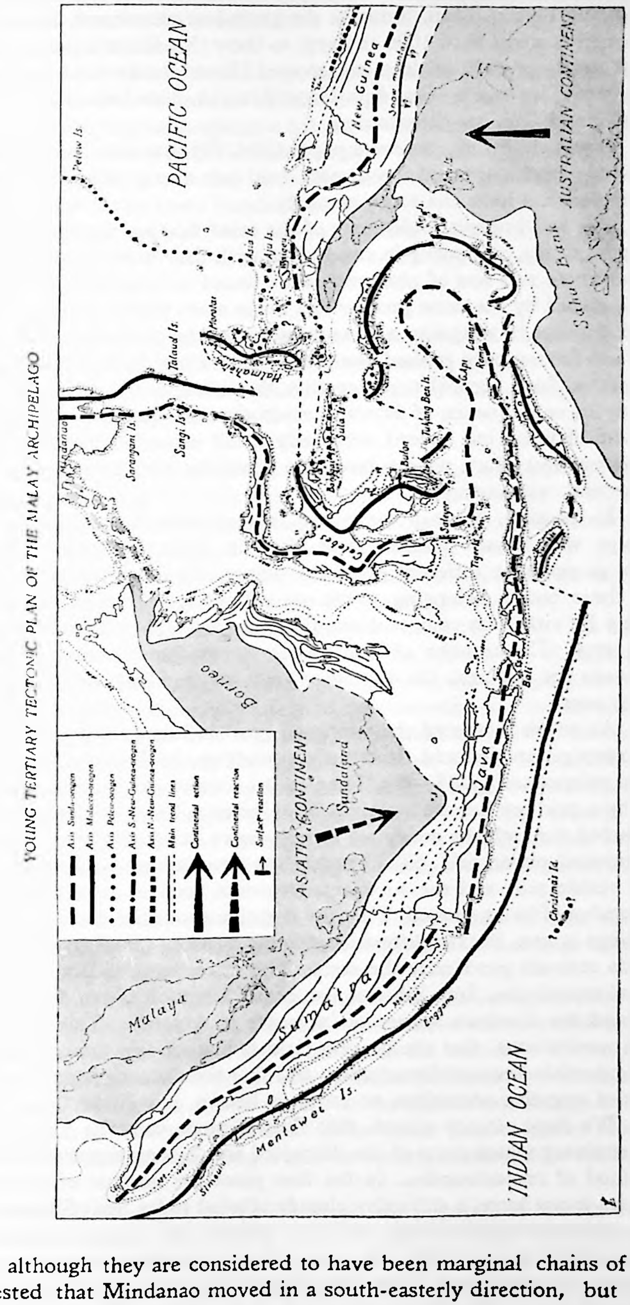


Fig. 46. Structural map of the East Indies by Smit Sibinga, bibl. 102.

The proposed arcs, moreover, are twice as long as the distance from Java to Mindanao, although they are considered to have been marginal chains of the Soenda land. Smit Sibinga suggested that Mindanao moved in a south-easterly direction, but this could only explain a small amount of difference between the present distance and the length of the arcs. How were they stretched to this extent without breaking?

Another objection to Smit Sibinga's reconstruction is that where the contrast between the Banggai Archipelago and east arm of Celebes, where a recent join is constructed, is emphasized, other contrasts between the islands on the same arc east of Timor are not touched upon. In the same way Peleng Strait, between the parts just mentioned, is called „merkwaardig" (remarkable), although it is less than 1000 m deep, to show that the connection is „posthumus", whereas in other parts straits of 4000 m deep are crossed (Inner Banda Arc), connections over many hundreds of kilometers are made (Gn. Api-Tijger Islands), and obvious morphological connections, as between Misool and Obi, are disregarded.

It was shown in a separate paper (bibl. 68) that there is a far reaching coincidence between the present morphology and the gravity field (see also p. 61). All basins with the same morphological characteristics have the same anomaly.

The less important elements of the relief find no counterpart in the gravity field, partly owing to lack of data, and partly in connection with their superficial character. The unsuspected degree in which these two sets of phenomena are found to hang together, warrants the assumption that they were caused by the same process and at the same time or directly after eachother. We cannot follow Smit Sibinga in assigning a cretaceous age to the anomalies and an upper tertiary age to the relief, the arcs having been pushed completely away from the belt of negative anomalies. Either the anomalies have been formed together with the relief, or the latter was formed after the anomalies and partly in consequence of isostatic readjustments. As the anomalies do not follow Smit Sibinga's structural lines, but accord accurately in all essential points with the connections visible on the bathymetrical chart, we are forced to conclude that the latter is a more trustworthy guide to the structural relationships.

In passing, it may be noted further, that the subdivision of the troughs into various groups was found possible, but that in Smit Sibinga's conception, the various members of each group have different positions among the positive structural elements. Thus, only two of the three basins belonging to the second group lie between the two arcs. The basins of the first group lie either far remote from the arcs, or beside one arc or within a loop of one arc, or between two arcs. The troughs of the fourth group are situated alongside one arc, but one part lies between the two arcs. On the other hand, we find three different kinds of basins between the two main arcs.

As to the matter of the supposed great influence of the Australian Continent, the inadequacy of many points deduced from the morphology, by Brouwer, to prove this assumption, has already been pointed out (p. 85—90). The parallel position of arc and continental border may also be explained by a primary formation as parallel elements. Proof is lacking that this parallelism was gradually assumed through the course of a progressive development: a pressing together (see also the above mentioned paper, bibl. 68). Whether the structure of the Outer Banda Arc is different in so far as the visible part and the submerged western section are concerned, as Smit Sibinga assumes, is incapable of being proved. That the negative anomalies decrease to the west, as pointed out by Smit Sibinga is true, but the difference existing between the negative anomalies and the adjoining positive fields remains practically the same. The disturbance indicated by the gravity field therefore is of equal magnitude. Smit Sibinga also draws attention to the disappearance of the negative anomalies beyond the northern sphere of influence of Australia. This point is admitted; however, with the two reservations, that the gravity field is known only from one section, which still shows a very considerable isostatic disturbance, and that the distance from the Australian mass to the end of the line of negative anomalies, so far as is known, is considerable.

We have already stated, that Smit Sibinga uses the magmatic differentiation as a means of ascertaining which parts of the disrupted arcs belong together. There are several objections to this method of reconstruction. In the first place the number of analyses of igneous rocks in the East Indies is not large, a difficulty already alluded to by Smit Sibinga. Moreover, the varying depth of the exposures renders very doubtful, whether the absence of certain types or rocks at the surface can be taken as proof, that the corresponding magma does not occur at greater depths. This objection is especially important, since Smit Sibinga only considers the matter of saturation with SiO_2, not the variation in the other components of the magma. When the latter come to be taken into account, a more reliable indication of the type of differentiation, and, therefore, of the distribution of mag-

matic provinces will be obtainable. In this respect the results obtained by Niggli and Burri should be called to mind (bibl. 28, 29).

Perhaps, on account of lack of data, the latter method cannot as yet be applied to the present problem, but then it appears doubtful whether the consideration of the silica saturation only, provides sufficient hold on the complicated matter.

Another objection to the method followed is, that although we may surmise that similar circumstances might produce similar differentiation, we have no right to turn the matter round and declare that similar differentiation proves that the regions in question belong to the same orogenetic element. If such were the case, we could make use of the recent volcanic activity of Halmahera to prove that it does not belong to the „Molukken orogen" but rather to the Soenda orogene; for, as far as knowledge goes, the products resemble those of the Inner Banda Arc.

To sum up: I believe there is little strength of proof in the arguments drawn from magmatic differentiation, by Smit Sibinga, although, on the whole, they fit in satisfactorily with his conception, with some exceptions.

As far as the geology of the islands is concerned Wegener's or Smit Sibinga's hypotheses can be said to present certain advantages, but the disadvantages are considerably greater.

To these objections must be added a number of theoretical considerations against the hypothesis that the arcs are floating strips of sial in a sima ocean.

These will now be put forward.

1) The bending of an arc (and the straightening out on release from the strain) implies tension without breaking. Structural geology teaches us, however, that tension is always followed by rupture. One transverse fault in an arc (and there must be many) would prohibit the bending (or subsequent straightening) by forming a sudden and permanent gaping fissure. We cannot bend a bow made of brittle material. If it is held at one end and bent back at another, in the manner Smit Sibinga explains the bending of the Outer Banda Arc, it would snap into two or three straight portions.

2) If the sial strips can float about ad libitum in the sima this must be highly fluid. The varied and steep submarine topography of most of the sima pools contradicts this conception of the nature of the sima. The deep between the Banda Arcs is 2500 meters greater than the normal uncovered sima. If the sima were so highly fluid it could not possibly fail to rise up between the floating islands.

3) Did the stripping off and warping of the arcs take place since the Miocene? If not, what caused the intensive tectonic compression of Timor? The small movement of this island through the sima cannot be considered as the cause, for other parts which were moved ten times as far (Celebes or Boeroe) are far less drastically compressed. Smit Sibinga only considers whether the pressure is direct or indirect, but that cannot be the criterion of greater or smaller stress having been exerted. It is the amount of movement that shows which part has been most severely attacked and pushed against.

4) In some parts the sial strip is about as broad as it is deep, namely some 50 km. Would one not expect, under the supposed physical circumstances, that a sialberg now and then toppled over or actually capsized?

5) To my mind a narrow mobile sial strip, with a row of active volcanoes along its axis, is a highly improbable conception. After the severance from the continent, the volcanoes continued to produce the same magma in the same degree of activity as the volcanoes on the continent. The magma contents of the volcanic chambers was neither violently projected nor drained off, nor even contaminated with sima, yet it must have been sufficient to render the arc at the time it was stripped from the continent like a bottle full of liquid, for the present islands of the Inner Banda Arc are almost entirely built up of materials produced since the drifting began and rest on an inconspicuous elevation of the sea floor.

6) As the Alps never consisted of sial strips in a sima ocean many points of resemblance between the two structures would be merely a coincidence. As the faults drawn by Smit Sibinga are entirely hypothetical and in part certainly wrong, they are no proof of a fundamental difference with the Alps. There are however, many other points of striking resemblance that cannot be regarded merely as „coincidence". (see p. 59).

To sum up the results of this section: I believe we may say that the drift theory, as applied to

the East Indies by *Wegener or Smit Sibinga only increase the difficulty of gaining a satisfactory conception of the development and present structure of the archipelago.* No doubt the only alternative explanation that can as yet be offered, namely of a mobile portion of the earth's crust that is being compressed, also offers many difficulties and apparent contradictions, but I believe I have shown that there are no convincing arguments to be obtained from the geological structure to favour the drift theory.

Curiously enough Wegener elsewhere in his treatise suggested an entirely different explanation of the Banda Arcs as if the problem had not been already dealt with. The spiral-shaped bend of the Fiji Group is explained as a kind of vortex in the sima, resulting from the drift of the Australian continent (bibl. 127, p. 216). Wegener goes on to say that the spiral at the other end of New Guinea, namely the Banda Arcs, can be explained in the same way. This hypothesis seems to me untenable, not only from a mechanical point of view, but also on account of the wrong position of the „vortex" in the Banda Sea with respect to the body that is supposed to have formed them: namely in front of it.

On the palaeontological evidence given by the rich marine fauna of the Permian of Timor, Gerth attempts to prove that the island cannot have been situated more than 30° from the Equator at that time. According to Wegener's reconstruction the distance must have been as much as 45°. Gerth holds that as there cannot have been an ice-covered pole as close to Timor as Wegener's theory implies, the palaeogeographical reconstructions must be wrong (bibl. 42). We must not forget, however, that the latitude can only be given very roughly for any spot in former periods and that the temperature in which a permian fauna lived is only roughly indicated. Finally, a warm current could very well have carried sub-tropical marine conditions far towards the pole. For these reasons Gerth's objections can only be considered as unfavourable to Wegener's reconstructions, but not as incompatible with them.

We will now pass on to the second question.

2. *Can a morphological dividing line be drawn between Australia and Asia?*

To answer this question we should start from the trough south of Timor, for if such a line is to be found it must doubtless be sought in this deep, elongated trough.

Towards the south-west the trough becomes narrower and shallower, reaching a minimum to the south of Roti with 2000 m. From there it deepens and widens again, bending round and continuing in the Java Trough. Here the first difficulty is encountered. The narrow and shallow part may be explained by a more intimate compression of arc and continent. It is not logical, however, that where the Australian continent ends towards the west, the seam where arc and continent were joined together, should continue along the edge of the arc beyond the limits of the continent. On the contrary, it appears that the trough belongs to the arc and only becomes shallower where the ridge rises up above the water, which is also where the ocean bottom to the south is replaced by the continent. The facts are strongly opposed to the conception that in the Timor Trough the scar is represented of a drifting-together. The trough appears to have been formed simultaneously with the island-chain, in the manner of a crack, not of a join.

If followed in the opposite direction the Timor Trough continues along the Outer Banda Arc to the Aroe Basin. Unfortunately there are no soundings to the north of the ridge, east of the Kai Islands. It is doubtful therefore, whether this ridge crosses the trough and is connected with the south coast of New Guinea (see fig. 5, Plate I). Although the trough looses its simple character further along the southeast border of the New Guinea coast and becomes subdivided by a few more and less distinct ridges, it can be clearly traced as far as Misool. From here onwards the trough becomes increasingly deeper, approaching 5000 m north of the west point of Ceram.

The further continuation of the seam between Asia and Australia, was first placed by Smit Sibinga somewhere east of Obi and Halmahera. Later he added the Palao Arc. Wegener, on the other hand, appears to have placed it on the west side of these islands, although he did not actually say so. We must therefore attempt to find the best position from the new chart, but it will be seen that no satisfactory solution can be found.

If, as seems most likely, the Ceram Trough continues to the deep north of Boeroe, the joining line would run on to this point and from there due north between the Soela Islands and Obi, the Batjan Deep and along the west coast of Halmahera and Morotai. This line, however, crosses the very distinct ridge between Mangole and Gomoemoe, south of Obi. Although this connection is composed

of two interchanging ridges its deepest point still lies 3000 m higher than the adjoining troughs. North of Morotai another very distinct ridge has to be crossed, connecting this island to the Snellius Ridge.

Our difficulties are not at an end here. Coming from the north the Mindanao Trough follows along the east slope of the Snellius Ridge and passes down east of Morotai, bending away to the east in the deep north of New Guinea in the latitude of 1°—30′ north. Therefore the only possibility is to assign a later date to the formation of this trough than to the advance of Halmahera and Morotai (as parts of the Australian continent).

The alternative position for the joining line onwards from the east end of Ceram must be to the north, somewhere east of Obi and Halmahera. Now if it is placed directly to the east of the latter island it crosses at least five ridges and ends vaguely in the Mindanao Trough where the latter alters its course from due south to due east. A slight improvement is found by taking a north easterly course along the north edge of the flat of New Guinea. In this case a ridge is crossed that connects Gomoemoe (south of Obi) with the shelf north of Misool. From there a trough can again be followed running south of Kofiau. A possible ridge between this island and Salawati is then obliquely cut. Finally, our joining line would cut between Batanta and Salawati and run on to the deep sea north of New Guinea. The strait between these two islands is only just over 200 m deep and therefore a rather surprisingly well closed join!

Any other position for the line seems out of the question and has never been proposed.

Reviewing the facts ascertained concerning the position of the joining line between Australia and Asia we have found the following to be the case.

A clear morphological dividing line does not exist. Whatever course is given to it, it crosses several ridges in the north and includes the southern continuation of the Mindanao Trough. Further, it follows a trough in the south which continues beyond the limits of the two parts joined together. Other troughs of exactly the same character occur in the Molukken, although they could not also be joining lines.

The only logical conclusion is that these morphological forms are younger than the process of the joining together of the continents. Not only do these features belong to the major lines of the entire structure (Obi-Soela Ridge, Morotai-Snellius Ridge, all the ridges connecting Halmahera to New Guinea, the Java- and Timor Troughs and probably the Mindanao Trough), but there is no reason to suppose they are younger or of a different age than all the ridges and troughs of the entire archipelago.

There is but one view tenable, namely that all the major features of the relief of the Molukken were developed after the arrival of the Australian block.

This very important conclusion concerning the relative age of the morphology and the drifting has a number of consequences. In the first place the features of the morphology cannot be a consequence of the arrival of Australia, nor can they have been influenced in their position or course by this continental drift. The warping of the arcs was not caused by the movements of Australia, as is supposed by the drift theory. Whether Australia drifted to its present position or lay there to begin with can have no influence on the development of the island arcs and deep-sea troughs. Australia already touched Asia before the troughs and island arcs were formed as such and cannot be made responsible for the bending in of the latter.

A second result is that Australia cannot have wandered to its present position as recently as Wegener's theory assumes. It must have arrived at the latest in the Miocene, for the island arcs were already produced in the Plio-Pleistocene at the very latest and probably began to form earlier.

A third consequence is that, as the troughs were formed after Australia arrived, they cannot be taken as a sure indication of the joining line. As some troughs and ridges cross the dividing line they may all do so. If therefore a dividing line is to be assumed it cannot be located on the strength of the morphology. Only geological data can be used. If these lead us to assume a position for the dividing line along a trough, then it is only a sign that in the formation of troughs and ridges the former tended to develop along the old line of weakness.

3. *Is Wegener's date for the joining of the continents sufficiently founded?*

Wegener assumed that Australia was still about 2000 km from its present position in the lower Quaternary. As far as I am able to ascertain, the grounds for this assumption are the following (bibl.

127, p. 111 etc.). The Australian fauna is composed of three elements. The oldest of these indicates a connection with Madagascar and British India which was interrupted about in the Lower Jurassic. The second element is formed by the primitive mammalia which prove a connection with South America and the Antarctic that was broken in the beginning of the Tertiary. The third element comes from the East Indies and arrived since the end of the Quarternary, taking possession of New Guinea and the Northern part of the continent.

As the distance travelled by Australia is about 4000 km it need not have taken longer than 2 million years to complete the journey, moving at a rate of 2 meters per year. If therefore the movement began in the Lower Eocene the joining could have taken place at any time from the beginning of the Oligocene onwards, as far as the distance is concerned.

The next point to be considered is whether the faunistic isolation proves a late date for the arrival. In my opinion it does not. The distribution of the Asiatic fauna in the East Indies clearly proves that the island arcs are by no means an easy highway along which the animals migrate freely. The fauna becomes increasingly poorer as we go further away from its source. Even on the islands which were connected by the Soenda land during the Ice Age the fauna is by no means identical.

How much more difficult must the migration be from one island to another when a deep passage intervenes! Even with the present formation the amount of interchange between Australia and the Molukken must be small, as is also proved by the comparatively small number of animals that has wandered towards the Archipelago from Australia.

If the obstructions to migration were greatly increased, practically no interchange of species would take place at all. This is amply proved by the case of Madagascar which is isolated from Africa, although the distance is only half as great as from Australia to the larger Soenda Islands.

Nearly all islands in the Eastern part of the Indies show signs of recent elevation, so that we can hardly be far wrong in supposing that in a comparatively recent period the island arcs were hardly anywhere above sea level. The volcanic islands are also very recent products. We might suppose that other islands existed where now there are deep troughs or submarine ridges. I believe, however, that all students of East Indian structural geology are convinced that the geanticlines, whether they are considered mobile or stationary, have at any rate been developing (possibly with temporary lapses) into increasingly larger elements since the upper Tertiary. This means that taken as a whole the amount of relief is increasing and that therefore the Archipelago consisted of smaller and fewer islands or was even almost entirely submarine at the close of the Tertiary period. Although the tertiary sediments prove that there must have been denuding islands, there is no neccessity for assuming the existance of migration lines crossing the entire East Indian region. There may well have existed breaks that were broad enough to prohibit all interchange between the continents.

If we apply the above reasonings to the migration of the fauna between Asia and Australia we see that the main features can be explained without assuming that the distance was formerly larger. The possibility of greater discontinuity or even absence of the island arcs in the Tertiary sufficiently explains the isolation of Australia, which is now being gradually overcome. The considerations in this section thus lead us to the conclusion that *the drift theory can be maintained for Australia, while we assume that the connection with Asia occurred some time since the Eocene, but that the theory is not necessary for explaining the isolation of Australia.* [1])

Finally, I wish to point out that if the intensive tectonic thrusting of Timor is to be explained by pressure exerted by the Australian continent, this block must already have touched the Indies in the Miocene, the period of the folding.

We now come to the last question:

4. *Can a geological dividing line be drawn between Asia and Australia?*

If we adhere rigorously to Wegener's theory we need only take into consideration the possibility of joining during the upper quarternary period. But in the former section we learnt that a less recent

[1]) It is admitted that the isolation of Australia is not the only migration problem of the East Indies. As I am unversed in biogeography these aspects will not be further considered here. It should be born in mind that my remarks only bear on the *geological* aspects of *one* problem of the drift theory and do not stand for a full treatment of the whole hypothesis.

connection is also possible and that the Oligocene, if any, seems the most likely period for the arrival.

It is in the nature of things that an answer to the above question cannot be precise. Only a small part of the area in question is above sea level and the geological knowledge of this part is far from complete. There is, moreover, a still graver obstacle to a satisfactory solution of this problem, namely that we do not know exactly how a geological boundary between two continents can be proved. No illustrations are needed to show that neighbouring parts on the same continent can show entirely different stratigraphy, structure and petrography. An alteration in one or all of these respects, however abrupt, cannot be taken as proof of a continental joining line. It would only show a possible position for this line. Another method is to seek what evidently *does* belong together and thus to eliminate a number of otherwise possible positions. In this way a „line of Wallace" has been sought for the biology. The latter search has proved vain: no such sharp line exists. As far as I can see the same is the case for a „line of Wegener."

We must turn to stratigraphy rather than to palaeontology for information. Distant fauna's may be similar, so that in order to establish former morphological connections we must also study the lithology of the deposits. Only by taking into account the whole facies of the rocks can we hope to find obvious connections between the parts under consideration.

The best starting point for our considerations appears to be the Outer Banda Arc. This arc is, and always has been, a geological unit. Not only does the morphological shape and also the tertiary tectonic history of the whole strip, as far as it is known, show a great regularity and a considerable degree of similarity throughout, but also in the Mesozoic the fundamental facts point to a connection of the whole area. Thus Wanner in his stratigraphical study of the mesozoic period for the East Indies (bibl. 125) says on page 568: „... äussere Molukkenbogen, der sich auch stratigraphisch als ein Zone von grosser Einheitlichkeit zu erkennen gibt..." He goes on to say: „Zu dieser Zone gehört auch Buton, Südost- und Ost-Celebes..." The tectonic-stratigraphic map in Umbgrove's article on the Neogene of the East Indies (bibl. 117, p. 831, bibl. 119) shows that for this period the arc is also one inseparable unit. Leupold and v. d. Vlerk (bibl. 75, p. 644) say: „The islands of the Outer Banda Arc present Tertiary sections that resemble eachother markedly."

The dividing line must be outside the Banda Arcs.

The islands which are most important for our investigation are Misool and Northern New Guinea. These belong to the Australian continent. As they are both situated on the Sahul Shelf, and as Mesozoic strata from both are known, there is a possibility of deciding whether they have been East Indian throughout their entire history, or only for the younger periods. The evidence is as clear as could possibly be expected and very decidedly shows a connection throughout the entire Mesozoic.

As early as 1921 Wanner expressed the opinion that Misool belonged to the same geosyncline as Ceram during the Mesozoic: „... kein Zweifel bestehen kann, dass das Mesozoikum dieser drei Inseln (Misol, Buru, Seran) in ein und demselben Geosynklinalbecken abgesetzt wurde." (bibl. 123, p. 164). In his recent article (bibl. 125) he does not expressly state the same opinion, but the information he gives and the comments he makes show that the new data have not altered the position.

The upper triassic strata of Misool and Ceram both consist of a Flysch-facies, followed by a shallow facies with many lamellibranchiata, and then by limestones in which Misolia plays an important part. The similarity is quite as pronounced as between Ceram and Boeroe for this period.

In the Jurassic the strata are in part almost identical. In the lower strata Misool shows affinity to the Soela Islands; in the higher strata the resemblance to Ceram is again pronounced. Wanner writes: (bibl. 125, p. 592) „... besonders im Malm die Uebereinstimmung mit dem Malm von Seram-Buru fast vollständig zu sein scheint."

In the Cretaceous Misool and Ceram again greatly resemble eachother. Hornstone-bearing, compact limestone with foraminifera is the most characteristic formation.

In the Tertiary the history of Misool appears to be linked up with the Soela Islands and Obi and with the southwest part of New Guinea (see Leupold and v. d. Vlerk, bibl. 75, p. 645).

Although the knowledge of the geology of New Guinea is scant, sufficient is known for our purpose. The jurassic strata of the Soela Islands and Obi are continued in the north part of this

island. Thus Wanner (bibl. 125, p. 595) writes: „Die cephalopodenreiche, geoden führende Tonschieferfazies der Sula-Inseln und von Obi erscheint wieder auf Neu-Guinea, wo sie von der Arfakhalbinsel bis an die Östliche Grenze des niederländischen Territoriums von einer ganzen Reihe von Punkten bekannt geworden ist." Also for the Cretaceous the resemblance to the same islands is marked. For the Tertiary all that can be said is that the geological history is very similar to that of other parts of the East Indies. The same type of sediment was formed with the same fossil contents. There is for instance a transgression in Tertiary e 5 as on almost all other larger islands of the East Indies (bibl. 75, p. 648).

Thus we have learnt that the geology of Misool and of Northern and Western New Guinea proves that these parts belonged to the Malayan geosyncline during the Mesozoic. Sometimes the resemblance is greatest between Ceram and Misool, sometimes between the Soela Islands and Misool, sometimes between the Soela Islands and New Guinea. This double and crossing link is as strong as could possibly be expected for portions of the same geosyncline a few hundred kilometers apart. For the Tertiary the data may not be so pronounced, but they certainly favour the above conclusion.

The advocates of the drift theory assume that Misool and New Guinea had a separate history at least up to the middle of the Tertiary. As parts of the Australian Continent, they lay at a distance of some 3000 km to the southwest. Moreover they had nothing to do with the Tethys-geosyncline. Advocates of drift must ascribe resemblance to parts of this geosyncline merely to coincidence. If there were only points in common with the Outer Banda Arc this might be looked upon as chance resemblance; now that the crossing link with the Soela Islands also exists, we can hardly explain the similarity without assuming that there are substantial reasons for this identical geological history. There is but one possible conclusion: Misool and New Guinea north and west of the Sneeuwgebergte did not belong to the Australian continent before it drifted up against the island arcs.

Two parts only now remain to be considered, namely the Aroe Islands and the south-east part of Dutch New Guinea.

Of the Aroe Islands comparatively little is known. Zwirzycki believes they belong to the Australian continent (bibl. 132, p. 315) and my own short investigations brought nothing to light that opposes this view. It is moreover the most probable assumption on account of the Australian character of the fauna and the position of the group.

Of the remaining part of New Guinea very little is known, not only on account of the unvaried character of the geology, but also on account of the scarcity of information as yet obtained. Zwirzycki is of opinion that the substratum forms part of the Australian continent.

The conclusion we arrive at is: If there is a dividing line between Australia and the East Indies, it must lie east of the Banda Arcs and south of the Sneeuwgebergte of New Guinea. Either there is no dividing line, and this is by far the most logical conclusion, or the dividing line has subsequently been entirely erased.

To this possibility, however, there is one apparently insurmountable difficulty, greater than the improbability just mentioned. The fauna and flora of New Guinea are absolutely and typically Australian and therefore flatly contradict the hypothesis that this island is composed of two parts, one Indian and one Australian. Neither can any support for this idea be found from the position of New Guinea on the surrounding sea bottom.

C) Tentatively proposed alteration of the drift theory with reference to the movement of the Australian Continent.

In the previous sections we have learnt that the drift theory, either in the form proposed by Wegener, or in that given to it by Smit Sibinga, does not give a satisfactory explanation of the structure of the East Indies, while important objections can be raised to the conception of the arcs as mobile strings of sialbergs in a sima ocean, of which those founded on the investigation of gravity are not the least convincing. We have also learnt that the main features of the morphological structure were generated after the arrival of the Australian continent, thus providing another strong argument against the sialberg hypothesis, and moreover excluding the possibility of a recent arrival of the continent. Then, it was proved from stratigraphical data that the northern part of the Australian continent belonged to the Malayan geosyncline since the beginning of the Mesozoic. For explaining the isolation a greater distance does not appear to be necessary.

It must be admitted that serious as the objections to the drifting of Australia may be, there are other strong arguments in its favour.

The two principal ones are, first, the Australian faunistic relations to British India, and to the Antarctic with South America, secondly, the possibility of explaining the permian ice age.

Must these advantages of the drift theory be discarded on account of the objections raised above, or can a compromise be made? It is not the place here to go into this matter in all details, but a possible way out of the difficulty will be put forward, as follows:

Australia and New Guinea with Misool always formed part of the same continent. This continent always had roughly the same position in relation to the East Indies. All the movement made by Australia was obtained by tectonic compression in the East Indies in the same manner as all the movement made by British India was obtained by tectonic compression in the central mountains of Asia.

It should be noted that on Wegener's map of the Carboniferous the distance from Australia to the East Indies is much smaller than in the Eocene. If the shape of the East Indies and the adjoining part of the Asiatic continent is altered it can be fitted in between New Guinea and British India, thus filling in the only gap in the continental slab presented by Wegener's map of the Carboniferous.

We must next suppose that when British India began to wander towards Asia it was divided from Australia in the same manner as it was severed from Africa. When later America drifted away westward it is shown on Wegener's map as taking Antarctica and Australia with it. According to our supposition, however, Australia remained more or less where it was and the rupture with Antarctica and South America took place as a result of the drifting away of the latter continent.

During the Tertiary, Australia moved up northwards and rotated in the opposite sense to the hands of a clock, keeping in contact with the East Indies.

Of the three possible interactions of continents as set forth at the beginning of this section, the last type was only represented by the case of Australia, namely that one continent drifts up against the other. In the proposed system this case is altered to the same type as that of British India and only two interactions remain, namely drifting apart or pressing together of already touching continents. The development of the East Indies is thus believed to be the same as of the tertiary chains of the Alps and central Asiatic mountains. The only difference is that in the case of the East Indies the last stage, namely the elevation of the structure has not yet been completed. The many points in which the East Indies resemble the Alps are not a mere coincidence but a natural consequence of the fact that both are believed to be the result of similar interactions of two approaching continental blocks.

I am far from claiming that this history of the East Indies and Australia is entirely satisfactory. The most doubtful points seem to be whether the structure of the East Indies is of such a nature that, flattened out to its original shape, it could move Australia sufficiently and in the correct sense to bring it beside the former position of Africa and India, and whether a satisfactory explanation of the gravity anomalies could be given. The only reason for proposing this bold and poorly founded hypothesis, is that it combines the most attractive elements in the drift theory as far as Australia is concerned, without showing the — to my mind insurmountable — obstacles of the original suggestions of Wegener for these parts. I must leave it to future investigations to prove whether the drift theory as dealing with Australia in the Palaeozoic and Mesozoic is correct and whether the new form is more acceptable for treating the tertiary history of these parts than the old form.

11. ISTHMIAN LINKS.

Ch. Schuchert and Bailey Willis have recently proposed a new theory to explain faunistic links between widely separated areas without assuming continental drift or the submergence of vast continental areas (bibl. 98 and 128). They suggest that the missing links were formed by narrow geanticlinal ridges, for which they propose the term isthmian links. Panama forms the type example. On Plate 28 Willis represents the East Indian isthmii for the Permian and a different suggestion is given on Pl. 29. Very little is known of the former distribution of the Permian in the East Indies.

As the present relief of the area is comparatively young it can certainly not give us any help whatever in constructing Palaeozoic morphological forms. The permian isthmii suggested can therefore only be accepted as illustrating a principle, not as indicating even roughly their actual position.

Although the principle of isthmian links can hardly be claimed to be new, it is certainly of great value that it has again been brought forward and shown to be an alternative to Wegener's theory. In the opinion of the present author it is especially important, when applying the principle to the East Indies, to weigh the possibility of former submergence of the present interrupted links to explain the isolation of Australia which is far more striking than its roads of communication. We are dealing with a link that has recently been established and may in future develop into a complete isthmain link.

Note added during proof reading, belonging to Chapter IV, 8, B p. 72—78.

In a recent article van Bemmelen describes what is believed to be an example of gliding from Java. (Ein Beispiel für Sekundärtektogenese auf Java, Geol. Rundschau, Bd. XXV, 1934, p. 175—194). As this example is based on detailed field examination it is of special importance. The block north of an E-W fault in a geotumor sank, while further north the foreland bulged up. Subsequently an E-W series of crescentic, spoon shaped faults formed and the bulge to the north grew in importance. There is much to be said for van Bemmelen's opinion, viz.: that the elevation of the foreland was caused by movements in the substratum, in consequence of the faulting.

The sediments of the bulged part show four distinct movements: 1. a pre-quarternary folding; 2. a post lower-quarternary folding; 3 and 4. a general doming in 2 phases. There is no proof that the second and third phase occurred simultaneously. Only of the last two is the connection with the faulting (and gliding) rendered probable by van Bemmelen. The second movement might also be ascribed to a horizontal compression that caused the geotumor-formation. In other words: there is no proof that the gliding had any other consequence than doming of the foreland. This in itself is an important working hypothesis that merrits full attention of geologists.

On the other hand it should be emphasized, that although the conditions for gliding appear fairly favourable (a considerable grade and a plastic substratum) the result, as far as proved, is an exceedingly slight tectonic phenomenon, and of a quite different nature to normal folding. Seen from this angle, van Bemmelen's exposition goes far to show that gliding is quite inadequate to produce normal orogenesis.

Van Bemmelen also cites an interesting article by Bain („Flowage folding", Amer. J. Science, Vol. 22, 1931, p. 503—530). The structures for which Bain claims an origin through flowage, are comparatively small and are believed to have been formed at some depth.

His arguments are of considerable weight. He also considers the possibility of explaining larger structures by flowage.

CHAPTER VI

NOTE ON THE ECHO SOUNDINGS TAKEN IN THE RED SEA AND THE INDIAN OCEAN

A number of echo soundings were taken on the voyage from Holland to Sumatra. Several very important discoveries were made.

In the *Red Sea* a bank which was reported, north of Jebel Teïr, was investigated. Although the depth was found to be several hundred meters, a phenomenon was discovered of vital importance to the interpretation of bathymetrical forms, namely that of submarine fault scarps. The technical and theoretical discussion of these will be found elsewhere in this volume (p. 24 and fig. 9). Here it is the geological position that claims our attention.

The number of sections sounded was too small to allow of a determination of the direction and extent of the faults. That they are parallel to the coast is quite possible. From the geology of the coasts, the general shape and the relations to the East African fault-zone, the conclusion had already been drawn that the Red Sea is either a fault trough or a fissure in the sial continent (see for instance bibl. 89). The discovery of submarine fault scarps with a displacement of 200—300 m adds considerable weight to this interpretation. Conversely, the interpretation of the submarine steps as faults is strengthened by the location of the features in a region where faults were to be expected.

A section was run through the entire Indian Ocean from 11½°N—51½°E at the African coast to Suvadiva in the Maladives and from here to Padang on the west coast of Sumatra.

The African continental shelf ends at a depth of 90 m, as follows from the soundings taken at distances of about 1 km: 91, 87, 89, 90, 119, 127, 178, 198, 237, 432, 437: (Plate IV, section 1).

At a distance of 150 km from the coast 4000 m was reached. The greatest depth of 5100 m was sounded at 8½°N—54½° E. Then followed a region with depths of 4500. The remainder of the section to Suvadiva was found to be very irregular. First three banks were found, 3320, 3860 and 3490 m deep. One of these started with a scarp of 500 m (Pl. III, section I) at 7½°N—56½°E. Onwards from 6°—22′N, 58°—26′E the depth varies from 3000—4000 m, with banks of about 2000 m (see section 3 of Plate IV). Another scarp with a drop of 600 m from 3590—4210 was formed at 6°—32′N, 58°—26′E (Pl. III, section II). The submarine section of Suvadiva is very gradual, except for the last 200 m.

We cannot decide from this single section whether any of these banks are submerged atolls. Crosswise sections to ascertain whether the surface is flat (it need not be horizontal) and soundings with a heavy sounding tube (see bibl. 65) to look for coral detritus below a surface layer of ooze would be necessary to decide this important theoretical question. One of the banks (righthand side of section 3, Plate IV), certainly presented the section we should expect to sound on a submerged atoll. Here is a fine subject for investigation by a future expedition.

Beyond Suvadiva to Sumatra the section is of an entirely different character. At the bottom of the slope of Suvadiva begins a dead flat expanse of about 4600 m over 500 km. The variations in depth hardly exceed the accuracy of the soundings. At 1° S from 89½°E to 90¾° E a plateau was found, 200 km broad, with an average depth of 2500 m (min. 2200 m). The following 600 km are again 4600 m deep and almost perfectly flat.

The remainder of the section is again varied. From 96° E onwards a plateau was sounded of

3500 m, some 40 km broad, followed by 20 km of depths around 4800. After a ridge of 4200 m and 6 km broad a trough is found, 90 km broad, with a maximum depth of 5500 m, close before the slope begins which runs upwards to Siberoet (section 2, Plate IV). The Mentawei Trough has a regular basin-shape, 1600 m in the middle.

Several interesting points claim our attention. At first sight these great flat expanses appear to fit in with the old conception that the ocean bottom is more level than the continents. However, large expanses with a relief of less than 100 m are also found on the continents. The North Sea, the Soenda Shelf, the Sahul Shelf, the basin of the Mississippi, the northern part of Germany are a few examples. The dividing plateau is 2000 m high and would if the water were withdrawn, represent a great highland plateau. Erosion would soon cut it into a formidable mountain range.

Thus the sections clearly demonstrate that, apart from erosional shapes, the Indian Ocean is no less varied in relief than the Continental blocks.

Nevertheless, the horizontal bottom represents a difficult problem. The horizontal parts of the continent are explained by base levelling, aided by swift sedimentation in the basins. For the deep-sea we must assume a different agent. Sedimentation is too slow and too regularly distributed to make an important difference, and erosion plays no part whatever.

It is also very doubtful whether the nature of the sima, as is often claimed, solves the problem. Almost complete fluidity would be required for the sima to adjust itself so accurately. If we assume this explanation the isostatic equilibrium must be as complete as the regularity of the depth. Wegener suggests that the higher portions are thin sial strips.

The east end of the section is also of some theoretical importance. It proves the existence of irregular elevations on the outside of the deep-sea trough in front of the island arc. The trough is 1000 m deeper than the neighbouring ocean bottom and forms the continuation of the Java Trough and Timor Trough. South of Java, the row of elevations, culminating in Christmas Island and Corona Reef outside the trough was already known. In slightly different proportions therefore the same elements are found west of Sumatra and south of Java, namely from the land outwards: a shallow trough followed by a ridge, with a deep-sea trough and an outer row of elevations before the ocean bottom is reached.

The slope from Siberoet down into the trough shows distinct steps below the 100 m line. The Mentawei Trough, on the other hand, has a regular ynclinal shape.

CHAPTER VII

SUMMARY OF THE PRINCIPAL CONCLUSIONS

In the course of the examination first of the morphological forms, then of the gravimetrical data (especially in bibl. 68) and, finally of the various tectonic theories proposed for the explanation of the present structure of the East Indies, a number of scattered conclusions were arrived at. In order that a clearer insight can be gained, it will now be attempted to assemble these and to formulate a tentative conception of the structural build and development of the East Indian archipelago.

Fieldwork on the various islands and former study of the depth chart has revealed the following regarding the tectonic history (see Rutten, bibl. 93 and Umbgrove, bibl. 119).

During the Mesozoic shallow and deep basins existed. During the Cretaceous an orogenetic period affected part of the area and probably resulted in the formation of complicated structures. During the Lower Tertiary there are no indications of the existence of deep basins. (It is reasonable to associate the disappearance of the strong relief of foregoing ages with the late mesozoic diastrophism).

The following cycle of diastrophism took place after the Eocene and before Tertiary e5.

During the Miocene a very intensive diastrophism affected a part of the eastern section of the archipelago. Overthrust structures developed and are now known from several islands of the Molukken. Umbgrove has recently shown, that for all these localities the period of folding was the same, comparatively short period, namely Tertiary f (f2?).

There followed a period of elevation and denudation which in turn gave way to a more active period that was characterized by block faulting, while in other parts the mono-geosynclines were folded. More or less in direct connection and continuation, the present geanticlinal rows of islands were elevated gradually to their present heights.

Direct evidence, as to the age of formation of the present deep depressions is not available. Nevertheless, there are two potent arguments, in favour of a comparatively recent development.

In the first place, Umbgrove points out, that there are no stratigraphical indications of deeper facies in any division of the geolog cal timescale, since the Mesozoic, anywhere in the entire archip elago. In the second place, Molengraaff drew attention to the fact, that the miocene folds of Timor are cut off, obliquely, by the present coast, which is directly fronted by the deep basins. These basins thus appear as the counterparts of the rising geanticlines to which they run parallel.

Brouwer is of a different opinion. He contends, that the tertiary diastrophism has continued more or less uninterrupted up to the present time. The troughs and geanticlines are permanent features, but moving relatively (in the case of the Banda Arcs) towards the foreland, in casu the Australian mass. The troughs are being gradually narrowed down and may eventually be entirely extinguished. These horizontal movements are restricted in general to the deeper layers of the crust which pass under the visible surface like a plank pushed forward under a table cloth.

Again a group of investigators believes that, formerly, the arcs were simpler in shape and that the advent of the drifting Australian continent has bent and buckled them into their present forms.

Our task has been to attempt to throw light on these problems by the examination of the new bathymetrical chart. The following deductions have been drawn.

The echo soundings have taught the existence of vertical and very steep scarps along the sea floor, which evidently represent fault scarps. The oblong troughs and transverse depressions of geanticlines that were formerly looked upon as the scar of faults in the earth's surface are of a different nature. The former may be either faulted troughs or large synclinal basins. The latter are partially

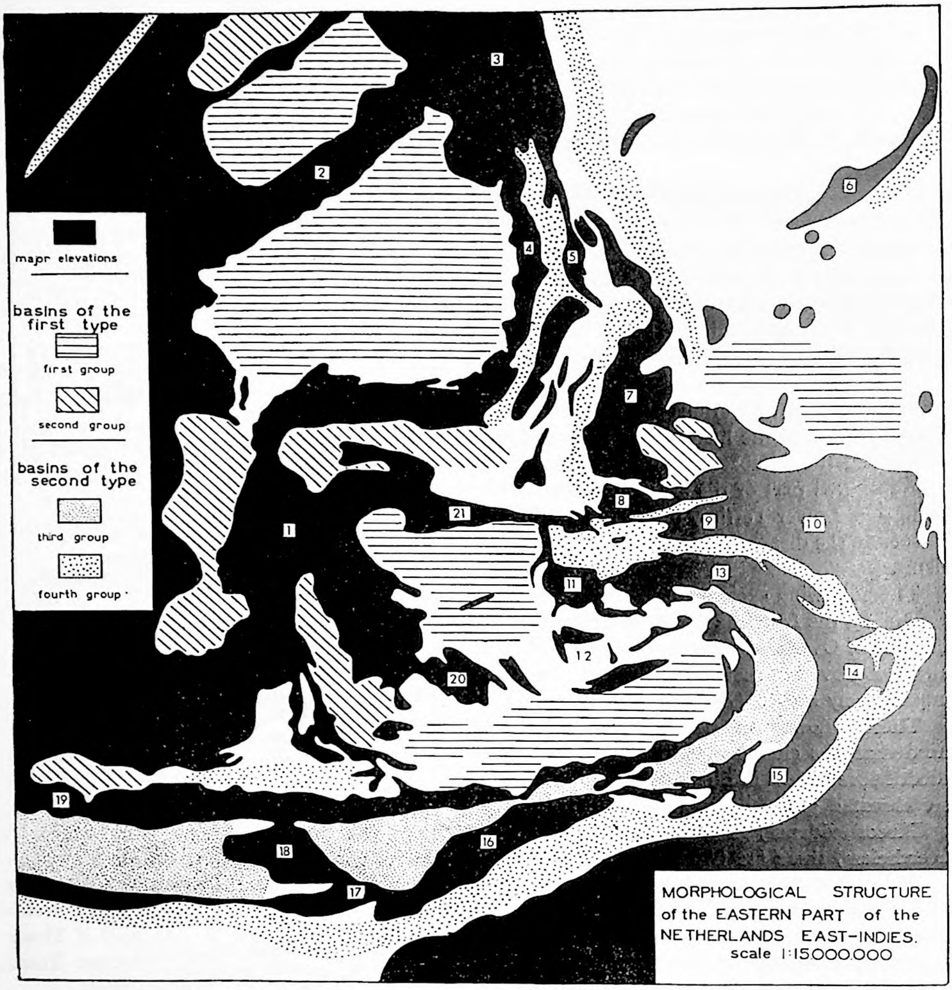


Fig. 47. Morphological structure of the eastern part of the East Indies.
1 = Celebes, 2 = Sibutu-Basilan Ridge, 3 = Mindanao, 4 = Sangihe Ridge, 5 = Talaud Ridge, 6 = Palao Ridge, 7 = Halmahera, 8 = Obi, 9 = Misool, 10 = New Guinea, 11 = Boeroe, 12 = Luymes and Siboga Ridges, 13 = Ceram, 14 = Kai, 15 = Jamdena, 16 = Timor, 17 = Sawoe, 18 = Soemba, 19 = Bali, 20 = Toekangbesi, 21 = Soela Islands. (Newer data disprove the existance of the formerly assumed „Palawan Trough").

the consequence of interchange of the crests, forming the culmination lines on the major geanticlines. In part, they appear to be depressions of the geanticlinal crests, as the consequence of pitch and/or of eroded (and stepped) transverse faults. The former type appears to be the more important as regards size and number. On the volcanic arcs the spaces between the volcanoes are probably primary, unfilled gaps. Some influence must also be attributed to the erosive power of currents and their prohibiting of sedimentation.

Consideration of the morphology of the larger basins has led to a subdivision in two main types. One is characterized by flat and horizontal floors and the other by a narrow synclinal shape, and the linking in long festoons of troughs having considerable variations in depth. Both types comprise two different groups. The groups of the first type are distinguished from eachother chiefly by greater size and depth and less oblong shape of the one in comparison with the other. Both groups are most readily explained by a regional downward force whereby they sank „en-bloc" along flexures and/or, stepped faults all around. All representatives of this type show a positive anomaly of the force of gravity that fits in with the explanation of their formation, just given. The groups of the second type are to be distinguished by the more typical synclinal shape of the one and their situation at opposite sides of the Outer Banda Arc and its continuation to the west. Their formation by a horizontal compressive force in the mode of normal synclines is not merely indicated by their general shape and position, but also by their parallellism to the intensively compressed orogenetic belt of the Outer Banda Arc, which also denotes the belt of strong negative anomalies of the force of gravity.

This conception finds further support by comparison of the basins with the fossil sedimentation troughs from which folded mountain chains have arisen. Both groups of the second type, but more particularly the typical synclinal troughs of the last group, conform in all details to the reconstructed basins which occurred in some of the orogenetic geosynclines.

As all geosynclines so far studied, have been formed by subsidence of continental areas [1]) it is quite reasonable to suppose that the present geosynclinal troughs of the Molukken, also originated by depression of a former continental area.

Although a satisfactory conclusion was not possible in regard to the basins of the first type, there are various indications that they are not comparable to the troughs from which folded mountains have been generated. They may be faulted graben or possibly „Innensenken."

If one was to attempt to reconstruct a former stage in the development of the archipelago, bearing in mind the deductions based of the morphology that have just been given, it is found that the basins of the first type, when raised to their original height, form one continuous mass with Celebes in the centre and joined on to the eastern and north-eastern edges of the Soenda continent. How much of this mass actually was dry land cannot, of course, be conjectured.

There remains only a comparatively narrow belt running parallel to the line of negative anomalies. The central part of this belt is now formed by the geanticlinal ridge of the Outer Banda Arc. The accompanying troughs, on account of their section, groundplan and general aspect are believed to have been formed, as broad synclinal depressions, out of a former shallow area.

So far the deductions based on the morphology are found to accord in a most satisfactory way with the views held by Molengraaff and Umbgrove. Taken all together the foregoing conclusions serve to indicate, that the Asiatic and the Australian continents at one time formed a more or less continuous mass. The mobile belt of alpine chains, running along the southwestern and southern edge of the Soenda continental mass traversed this continent at its narrowest point to continue north along the eastern edge of the continent of Asia. This behaviour is comparable with the way in which the same belt passes into the interior of the Asiatic continent in British India. As a whole, the mobile belt is therefore a marginal phenomena of the Asiatic Continent A virgation occurs at the southeastern corner, where the belt makes the strong loop of the Banda Arcs. This virgation passes through the northeastern and southeastern arms of Celebes. The orogenetic belt of New Guinea, in all probability, should be considered as being a more or less separate tectonic element.

Subsequently to the compression of the mobile belt, a number of parallel major anticlines and synclines were formed, while a number of deep basins developed through vertical subsidence. In the conception formulated by Brouwer, it is supposed that the development of the structure is a gradual process and that the present arcs are in the act of moving forward up against the Australian block. The Outer Banda Arc is the surface expression of the continually moving thrust-structures in deeper layers of the crust. A number of objections to this theory follow from the examination of

---

[1]) All Schuchert's types of geosynclines with the exception of the „meso-geosynclines". Since the type example of the latter, the Alpine geosyncline was formed on a continental area also (See Albert Heim, bibl. 49) there would seem to be no exceptions to the general rule of continental origin.

the new chart. The reader is referred to the section dealing with these matters for details (p —). In addition to Stille's arguments, it should be noted: that Brouwer's „bending points", with faults and anomalous strikes, have been replaced by interchanges of geanticlinal crests (Babar), or straight lines (Morotai); that tilted reef caps occur on „fixed" islands (Salajar), and anomalous strikes on comparatively straight parts of the arc (Ceram, Timor); that Kisar does not form part of the outer arc; that the arc does not bulge around the western border of the Australian continent; that the indenture of the continent opposite Jamdena has disappeared; that the arc does not bend into the indenture opposite the Kai Group.

Apart from the objections arising from the shape of the arc, the cross-section of the trough between arc and continent is not in favour of this view. In Brouwer's conception the inner slope of the Timor-Ceram Trough denotes the surface expression of the front portion of the moving geanticline and the outer slope is the edge of the resisting continent, whilst the bottom of the trough represents either the overrun edge of the continent or the remains of what was formerly a much broader geosyncline. As regards the section of such a trough, one would expect to find it either asymmetrical with a steeper slope on the side of the arc, or having a flat bottom and steep sides. The actual shape can be seen either on Plate VI, section 42—46 or on reference to fig. 22 and 42 in which the available sections have been drawn to a vertical scale 10× the horizontal scale. Not only is the shape typically symmetrical, but the shape keeps practically the same both at the broader and the narrower parts of the trough: that is, in Brouwer's conception, both in those parts where the two elements are still rather wide apart from eachother and those where they have already been pressed together. The asymmetry of the compressed sections is so very slight as to be of no practical importance.

The Tanimbar Group which springs forward, is also closer to the continent, whereas in the case of gradual adaptation, the distance would be greater, as then the outward bend would have been formed in consequence of a lesser resistance.

The synchronism of the intensive movements of all visible exposures contradicts the theory of gradual and continuous movements. The shallow facies of the youngest sediments of the islands is opposed to the view, that they were deposited in a deep trough and subsequently raised by the anticlinal wave passing beneath them. The fact that the most outlying parts of the present geanticline (Jamdena, Kai) bear evidence of intensive diastrophism during the miocene orogenesis, shows that the movements already reached at least as far towards the foreland as the present position of the geanticline. Diastrophism has therefore not approached the foreland (relatively) since the older stage.

These points demonstrate conclusively, that the existing adaptation is not secondary but primary and that the trough is a slight synclinal formation newly formed, not a semi-overrun trough or continental borderland.

We have still to consider the part that was played by the Australian continental block. In order to explain the compression in the mobile belt it must be assumed, that it has decreased considerably in breadth; that it was pressed together. The two continents in this way approached eachother. As, however, parts of the Australian mass belong to the same mesozoic sedimentation basins as the Molukken Arcs, this continent did not drift a very great distance before it finally established a contact, but always lay in its present relative position.

Various other considerations, in particular the morphological forms lead to the same conclusion.

An even more pronounced opinion, which can be expressed as: the absence of any appreciable influence of the Australian mass on the tectonic phenomena, is come to after the following facts have been duly weighed. In following the strike of the orogenetic belt along the southern margin of the Soenda Continent, from west to east, no new elements are found to enter opposite to the Australian mass. Neither the size nor the shape appear to be influenced; there is merely a change in the levels in keeping with the rise from sea bottom to continent running along the southern side. The disturbance of isostasy likewise remains constant, decreasing only over a short distance, opposite Soemba.

Of the tectonic structure of the Inner Banda Arc nothing is known opposite to Australia; neither is that of the Outer Banda Arc known for the part beyond the continent, so that no direct evidence can be obtained from this source.

The present relief may, or may not, indicate the position of the not exposed miocene structural

elements sothat we cannot be sure whether Australia had an important influence during that period. Whatever the case may be, the present shape, the result of the more recent movements, likewise demonstrates the lack of any important influence exercised by the Australian continent.

For although the Outer Banda Arc opposite Australia shows minor complications in general structure, the explanation cannot be found in the gradual adaptation of the shape of the arc to the edge of the continent, but must rather be considered to result from primary interchanges of the crests of secondary geanticlines (see above).

These observations lead one to assume that the tectonic phenomena are influenced only in minor degree by the presence of the Australian block. The bottom of the Indian Ocean and this continental mass have offered the same degree of resistance to the orogenetic stress coming from the Asiatic continent.

Our conclusions concerning the shape of the Outer Banda Arc are not opposed to the principle of adaptation of orogenetic systems to the forms and the relative degrees of resistance of the foreland (Alps and Jura). We are only able to judge of the results of the most recent movements resulting in the present morphology. By stressing the probability that these broad undulations were formed in parallellism, to begin with, we do not wish to imply that the older buried structure cannot show *local* adaptations. If the present major elements are subjected to further compression, they may in like manner very well attain a more complete adaption than they have reached at present. In my opinion the deviations from a straight line, or even from an even curvature, were, however, much greater at the time of first formation than are the differences between that first shape and the present irregular course.

It was already pointed out that Umbgrove proved in respect of all the parts of the Banda Arcs and Celebes having overthrust structures that the time of diastrophism was the same. There followed a period of less intensive orogenetic activity, the results of which are only indicated as yet by block-faulting at the surface of the islands and by the production of the present arcs and basins. There is lacking, however, the continuous and gradual development as postulated by Brouwer. On the contrary, Stille's opinion, that there are restricted periods of orogenesis divided by epirogenetic periods, is substantiated. If this be indead the correct interpretation of the structure, then the present relief corresponds to a comparatively slight compression. Drawn to scale, the section of the arcs and troughs represents but a slight undulation that would have been attained by a relatively inappreciable amount of compression. A rough calculation shows that in order to produce the present section of the Timor Trough, a compression amounting to merely 0,1 % of the original breadth would be required. This slight compression could not possibly have resulted in an adaptation of an evenly curved arc to a complicated shape. In the latter case the *relative* movements of different parts of the arc would already have to be 100 km at least. The conception of the recent formation of the present relief, which in first instance we owe to Molengraaff[1]) and which has been given a new formulation by Umbgrove, on the strength of extensive recent stratigraphical results, would appear to be irreconcilable with the notion of gradual adaptation of the arcs to the foreland. Our own results obtained from the examination of the morphological shapes, has led independently to the very conclusion necessary to fit that conception of Molengraaff and Umbgrove, to the conclusion namely that the present relief is a newly developed phenomenon.

Some geotectonic theories on the East Indies make an attempt at offering an explanation of the arcuate shape of the island festoons, either by development out of a formerly straight line or by the intersection of flat structural plains with the sphere of the earth, but they none of them appear to be really satisfactory. The arcuate shape therefore remains a baffling problem. This problem, however, does not appertain to island arcs only but in like manner to arcuate mountain chains. If the island arcs, such as those of the East Indies, are actually an orogenetic system, in statu nascendi, then only those attempts at explaining the crescentic shape of the rows of islands can be of value,

1) Molengraaff attributed a great influence to faults in the formation of the troughs. It will be evident from the foregoing that in this respect I differ somewhat from Molengraaff.

that can be applied directly to alpine mountain chains as well. For the time being, we must be content with noting, that orogenetic systems in most cases are found to present an arcuate ground plan.

As regards the question of what is taking place at present in the deeper strata of the crust, we are treading upon very unsafe ground. From the section of the troughs, and the unwarped shape of elevated reef terraces as far as knowledge goes, I am inclined to believe that neither thrusting nor blockfaulting is taking place now at depths, save possibly a vertical movement in the basins of the first type. It would not seam to be impossible, however, that the great crust fold of Vening Meinesz is in course of active development. Then the corresponding compression of the upper layers of the crust must be greater than is indicated by the bending into troughs and geanticlines.

The comparison of the East Indies with the Alps, which was first made by Argand is of great importance to tectonic geology. The alpine structure teaches us what the East Indian sub-structure is like, and the East Indies illustrate a former stage of the alpine orogenesis. In one respect, however, there appears to be an important difference. During considerable periods of the development of the Alps, the slope of the island arcs was so steep, on the convex side, that coarse detritus was delivered into the accompanying trough. The bottom sampling in the East Indies has not revealed a similar process. If the present rising of the islands continues, and is hastened, the conditions may, however, be altered in favour of coarser sedimentation.

The examination of the submarine slopes of the strato-volcanoes rising straight up from the sea floor has shown, that the declivity is uninfluenced by the height of the cone, either above or below sea level. The subaerial part is distinctly concave, only for the higher mountains, increasing with the height; the sub-marine slopes on the other hand are practically straight. These relations prove conclusively, that the concavity of the dry slope of volcanoes is a consequence of subaerial erosion. Variations in the force of the eruptions and in the size of the particles, play but a subordinate part.

Measurements of the slope at the stations where bottom samples were obtained during the expedition, led to the unexpected result that very mobile sediments accumulate in layers of more than 2 meters thick on slopes of at least 10—15°. The theory regarding the importance of submarine landslides for the filling of sedimentation troughs, should consequently be accepted with reserve.

These numeric data also caution in accepting the postulates of the sliding theories of Haarmann c.s., although they are not definitely opposed to the conception. Other objections, however, were deduced from the sections and groundplan of minutely examined orogenetic systems, in connection with the results one would expect sliding to have in a region such as the present East Indian Archipelago. The influence of gravity on the formation of folded and thrust structures is therefore considered to be on the whole relatively unimportant.

Leiden, July 1934.

VIII. BIBLIOGRAPHY

1. A b e n d a n o n, E. C. Die Grossfalten der Erdrinde. 1914.

2 A n d r e e, K. Geologie des Meeresbodens. Bd. 2, 1920.

3 A r g a n d, E. Sur l'arc des Alpes occidentales. Eclog. Geol. Helv., 14, 1916 p. 145—191.

4. A r g a n d, E. La tectonique de l'Asie. Comp. Rend. 13 Congr. Geol. Intern., 1922, p. 171—372.

5. A u s s p r a c h e Aussprache über die Oxillationstheorie. Z. d. deutsch. geol. Ges., 83, 1931, p. 257—388.

6. B e c k e r, G. F. The Geometrical Form of Volcanic Cones and the Elastic Limit of Lava. Amer. Journ. Sc., 30, 1885, p. 283.

7. B e c k e r, G. F. Form of Volcanoes, in: Reconnaissance of the Gold Fields of southern Alaska. U.S.G.S., 18th Ann. Rep., Pt. 3, 1898, p. 20—25.

8. B e c k e r, G. F. A feature of Mayon Volcano. Proc. Washington Acad. Sc., 7, 1905, p. 277.

9. B e m m e l e n, R. W. v. De bicausaliteit der bodembewegingen. Nat. Tijdschr. Ned. Indië, 91, 1931, p. 363—413.

10. B e m m e l e n, R. W. v. Het Boekit Mapas-Pematang Semoet vulkanisme. Verh. Geol. Mijnbk. Gen. Nederl. en Kol., Geol. ser., 9, 1931, p. 57—76.

11. B e m m e l e n, R. W. v. De undatie-theorie. Nat. Tijdschr. Ned. Indië, 92, 1932, p. 85—242.

12. B e m m e l e n, R. W. v. Ueber die möglichen Ursachen der Undationen der Erdkruste. Proc. Kon. Akad. Wet. Amsterdam, 35, 1932, p. 392—399.

13. B e m m e l e n, R. W. v. On the geophysical foundations of the Undation-theory. Proc. Kon. Akad. Wet. Amsterdam, 36, 1933, p. 337—343.

14. B e m m e l e n, R. W. v. Die neogene Struktulr des Malayischen Archipels nach der Undationstheorie Proc. Kon. Akad. Wet. Amsterdam, 36, 1933, p. 888—897.

15. B e r g e a t, A. Plutonismus und Vukanismus, in: Salomon, Grundzüge der Geologie, Bd. 1.

16. B o r n, A. Ueber Werden und Zerfall von Kontinentalschollen. Fortschr. d. Geol. u. Pal., Bd. 10, L. 32, 1933, p. 347—422.

17. B r o u w e r, H. A. Ueber Gebirgsbildung und Vulkanismus in den Molukken. Geol. Rundschau, 8, 1917, p. 197—209.

18. B r o u w e r, H. A. On the Non-existence of Active Volcanoes between Pantar and Dammer etc. Proc. Kon. Akad. Wet. Amsterdam, 21, 1919, p. 795—802.

19. B r o u w e r, H. A. On Reef caps. Proc. Kon. Akad. Wet. Amsterdam, 21, 1919, p. 816—826.

20. B r o u w e r, H. A. On the Crustal Movements in the region of the curving rows of Islands in the Eastern Part of the East-Indian Archipelago. Proc. Kon. Akad. Wet. Amsterdam, 22, 1920, p. 772—782.

21. B r o u w e r, H. A. Ueber die Horizontale Bewegung der Inselreihen in den Molukken. Nachr. Ges. Wiss. Göttingen, 1920, p. 197—209.

22. **Brouwer, H. A.** Fractures and Faults near the Surface of Moving Geanticlines I. Proc. Kon. Akad. Wet. Amsterdam, 23, 1921, p. 570—576. II, ibid., 25, 1923, p. 327—334.

23. **Brouwer, H. A.** The horizontal movements of geanticlines and the fractures near their surface. Journ. of Geol., 29, 1921, p. 510—577.

24. **Brouwer, H. A.** The Geology of the Netherlands East Indies. 1925.

25. **Brouwer, H. A.** Geologische onderzoekingen op het eiland Celebes. Verh. Geol. Mijnbk. Gen. Nederl. en Kol. ,Geol. Ser., 10, 1934, p. 39—218.

26. **Bubnoff, S. v.** Grundprobleme der Geologie. 1931.

27. **Buning, W. L.** De Geologie van den Cimone di Margno en den Monte di Muggio. Leidsch Geol. Med., 4, 1932, p. 321—399.

28. **Burri, C. R.** Chemismus und provinziale Verhältnisse der jungeruptiven Gesteine des pazifischen Ozeans und seiner Umrandung. Schweiz min. petr. Mitt., 6, 1925, p. 115—199.

29. **Burri, C. R.** Kritische Zusammenfassung unserer Kenntnisse über die Differentiationstypen postmesozoischer Vulkangebiete. Schweiz. min. petr. Mitt., 7, 1927, p. 254—310.

30. **Cadisch, J.** Das Werden der Alpen im Spiegel der Vorlandsedimentation. Geol. Rundschau, 19, 1928, p. 105—119.

31. **Chamberlin, R. T.** Isostasy from the geological point of view. Journ. of Geol., 39, 1931, p. 1—23

32. **Cornelius, H. P.** Zur Vorgeschichte der Alpenfaltung. Geol. Rundschau, 16, 1925, p. 350—377, 417—434.

33. **Dacque, E.** Grundlagen und Methoden der Paläogeographie, 1915.

34. **Daly, R. A.** Our Mobile Earth. 1926.

35. **Davis, W. M.** The coral reef problem. Amer. Geogr. Soc., Special Publ. 9, 1928.

36. **Es, L. J. C. v.** De Tektoniek als basis voor de opsporing van ertsen en fossiele brandstoffen. Alg. Ing. Congr. Batavia, 1920, 5 sec.

37. **Es, L. J. C. v.** The Age of Pithecanthropus. (Dissert. Delft), 1931.

38. **Escher, B. G.** Beschouwingen over het opvullings-mechanisme van diepzee slenken. Verh. Geol. Mijnbk. Gen. Nederl. en Kol., Geol. ser., 3, 1916, p. 79—88.

39. **Escher, B. G.** De Kloet van een geomorfologisch standpunt beschouwd. Nat. Tijdschr. Ned. Indië, 79, 1920, p.120—127.

40. **Gardiner, J. St.** The Indian Ocean. Geogr. Journ., 23, 1906, p. 313—332, 454—471.

41. **Gardiner, J. St.** Submarine slopes. Geogr. Journ. 45, 1915, p. 202—219.

42. **Gerth, H.** Die Korallenfauna des Perms von Timor und die permische Vereisung. Leidsche Geol. Med., 2, 1926, p. 7—24.

43. **Grabau, A. W.** Migration of geosynclines. Bull. Geol. Soc. China, 3, 1924, p. 207—349.

44. **Gregory, J. W.** The Rift Valleys and Geology of East Africa, 1921.

45. **Haarmann, E.** Die Oscillationstheorie, 1930.

46. **Hadding, A.** On subaqueous Slides. Geol. Fören. Förh., 53, 1931, p. 377—393.

47. **Hahn, F. F.** Untermeerische Gleitungen bei Trenton Fall etc. N.J. f. Min. etc., B.. Bd. 36, 1913, p. 1—41.

48. **Haug, E.** Les géosynclinaux et les aires continentales. Bull. Soc. géol. France, 28, 1900, p. 617—711.

49. **Heim, Alb.** Geologie der Schweiz. Bd. II, 1921, 1922.

50. **Heim, Arn.** Ueber rezente und fossile subaquatische Rutschungen und deren lithologische Bedeutung. N. J. f. Min. Etc., 1908, 2, p. 136—157.

51. Heim, Arn. Ueber submarine Denudation und chemische Sedimente. Geol. Rundschau, 15, 1924, p. 1—47.

52. Hess, H. H. Interpretation of geological and geophysical observations. In: The Navy-Princeton gravity expedition to the West Indies in 1932. U.S. Hydrographic Office, 1933.

53. Hess, H. H. Interpretation of gravity-anomalies and sounding profiles obtained in the West Indies in 1932. Trans. Amer. Geoph. Un. Nat. Res. Council, 1932, p. 26—33.

54. Hetzel, H. W. Over de geologie der Toekang-Besi eilanden. De Mijningenieur, 9, 1930, p. 51—53.

55. Hobbs, W. H. The unstable middle section of the island arcs. Verh. Geol. Mijnbk. Gen. Nederl. en Kol., Geol. Ser. 8, 1925.

56. Hobbs, W. H. Stress conditions within the lithosphere as revealed by earthquakes. Bull. Geol. Soc. Amer., 41, 1930, p. 739—746.

57. Höfer, H. Das polynesische alteozäne Festland. Sitzb. K. Akad. d. Wiss., Wien, 117, Abt. 1, 1908, p. 513—518.

58. Horn, E. Ueber die geologische Bedeutung der Tiefseegräben. Geol. Rundschau, 5, 1915, p. 422—448.

59. Jensen, H. I. The geology of Samoa and the eruptions of Savaii. Proc. Lin. Soc. N. S. Wales, 31, 1906, p. 665.

60. Judd, J. W. Volcanoes, 1881.

61. Kemmerling, G. L. L. De uitbarsting van den G. Kloet etc. Vulkanol. Med. nr. 2, Dienst Mijnw. Nederl. Indië, 1921.

62. Kober, L. Das Weltbild der Erdgeschichte, 1932.

63. Kon.Ned.Aardr.Gen. De zeeën van Nederlandsch Oost-Indië, 1922.

64. Koto, B. The Rocky Mountain Arcs in Eastern Asia. Journ. Fac. Sc. Tokyo, Sec. 2, Vol. 3,3, 1931, p. 131—183.

65. Kuenen, Ph. H. Die Viermeter-Lotröhre der „Snellius"-Expedition. Ann. d. Hydrogr. u. Mar. Meteor., 1932, p. 93.

66. Kuenen, Ph. H. Remarks on the undation-theory of van Bemmelen. Proc. Kon. Akad. Wet. Amsterdam, 35, 1932, p. 1155—1160.

67. Kuenen, Ph. H. Geology of coral reefs. The Snellius-Expedition, Vol. 5, Part. 2, 1933.

68. Kuenen, Ph. H. Eperiments on the formation of volcanic cones. Leidsche Geol. Med., 6, 1934, p. 99—118.

69. Kuenen, Ph. H. In bibl. 120; Chapter VIII, p. 183—194. Relations between submarine topography and gravity field. 1934.

70. Lake, Ph. and Rastall, R. H. A textbook of geology. 1927

71. Lake, Ph. Island arcs and mountain building. Geogr. Journ., 78, 1931, p. 149—160.

72. Lawson, A. C. The geological implications of isostasy. Bull. Nat. Res. Counc. 8, 1924, p. 10—13

73. Lawson, A. C. Insular arcs, foredeeps, and geosynclinal seas of the Asiatic coast. Bull. Geol. Soc. Amer., 43, 1932, p. 353—381.

74. Leuchs, K. Tiefseegräben und Geosynklinalen. N. J. f. Min. etc., Bl. Bd. 58, B, 1927, p. 273—294.

75. Leupold, W. and Vlerk, I. M. v. d. Tertiary. Leidsche Geol. Med., 5, 1931, p. 611—648.

76. Linck, G. Ueber die äussere Form und den inneren Bau der Vulkane. N. J. f. Min. etc., Festb. 1907, p. 91—114.

77. Marshall, P. Ozeania. Handb. d. reg. Geol., 7, 2, 1911.

78. M a r t i n, K. Reisen in den Molukken, Geologischer Teil. 1897.

79. M a u r e r, H. Echolotung bei geneigtem und stark bewegtem Bodenprofiel. Ann. d. Hydr. u. Mar. Meteorl., 1926, p. 336—340.

80. M a u r e r, H. Die Echolotungen des „Meteor". Wissenschaftliche Ergebnisse der deutschen Atlantischen Expedition auf dem Forschungs- und Vermessungsschiff „Meteor" 1925—1927. Bd. II, 1933.

81. M i l n e, J. On the form of Volcanoes. Geol. Mag., 1878, p. 337—345.

82. M o l e n g r a a f f, G. A. F. On recent crustal movements in the island of Timor and their bearing on the geological history of the East-Indian archipelago. Proc. Kon. Akad. Wet. Amsterdam, 15, 1912, p. 224—235.

83. M o l e n g r a a f f, G. A. F. Folded mountain chains, overthrust sheets and blockfaulted mountains in the East Indian archipelago. Comp. Rend. 12 Congr. Geol. Intern., 1913, p. 689—702.

84. M o l e n g r a a f f, G. A. F. On Manganese Nodules in Mesozoic Deep sea deposits of Dutch Timor. Proc. Kon. Akad. Wet. Amsterdam, 23. 1922, p. 997—1011.

85. M o l e n g r a a f f, G. A. F. De geologie der zeeën van den O. I. Archipel, in bibl. 63, 1922, p. 272—357.

86. M o r l e y D a v i s, A. A Note on Isostasy. Geol. Mag., 1918, p. 125—127. see also ibid., p. 192 and 233.

87. P i c a r d, L. Tektonische Entwicklungsphasen im nördlichen Palästina. Zeitschr. d. deutsch. Geol. Ges., 83, 1931, p. 164—184.

88. P r u v o s t, P. Sedimentation et subsidence. Soc. géol. France. Livre jubilaire, 1930, p. 545—564.

89. R a t h j e n s, K. und W i s s m a n n, H. v. Morphologische Probleme im Graben des Roten Meeres. Petermanns. Mitt., 79, 1933, p. 113—117.

90. R e y e r, E. Geologische Prinzipienfragen. 1907.

91. R i e l, P. M. v. The bottom configuration in relation to the flow of the bottom water. The Snellius-Expedition, Vol. II, Part 2, Chapter II, 1934.

92 R u d e, G. T. Hydrographic data. Bull. Geol. Soc. Amer., 44, 1933, p. 517—527.

93. R u t t e n, L. M. R. Voordrachten over des geologie van Nederlandsch Oost Indië, 1927.

94. R u u d, S. Entwicklung der Ostaiatische Gebirgsbogen. Petermanns Mitt. 75, 1929, p. 230—234.

95. S c h a f f e r, F. X. Ueber subaquatische Rutschungen. Centr. f. Min. etc., 1916, p. 22—24.

96. S c h m i d t, C. Ueber die Geologie des Simplongebietes und die Tektonik der Schweizeralpen. Eclog. Geol. Helv., 9, 1907, p. 484—584.

97. S c h u c h e r t, Ch. Sites and nature of the North American geosynclines. Bull. Geol. Soc. Amer., 34, 1923, p. 151—230.

98. S c h u c h e r t. Ch. Gondwana land bridges. Bull. Geol. Soc. Amer., 43, 1932, p. 875—916.

99. S h e p h a r d, F. P. Origin of continental abyssal slopes. Bull. Geol. Soc. Amer., 40, 1929, p. 107.

100. S h e p h a r d, F. P. Canyons off the New England Coast. Amer. Journ. Sc., 27, 1934. p. 24—36.

101. S m i t S i b i n g a, G. L. Wegener's theorie en het ontstaan van den Oostelijken O. I. Archipel. Tijdschr. Aardr. Gen., 44, 1927, p. 7 581—598.

102. S m i t S i b i n g a, G. L. The Malay double (triple) orogen. Proc. Kon. Akad. Wet, 36, 1933, p. 202—210, 323—330, 447—453.

103. S o l l a s, W. J. The Figure of the Earth. Quart. Journ. Geol. Soc. London, 59, 1903, p. 180—188.

104. S t a u b, R. Der Bau der Alpen. Beitr. Geol. Karte d. Schweiz. N. F., Lf. 52, 1924.

105. S t a u b, R. Der Bewegungsmechanismus der Erde. 1928.

106. S t e i n m a n n, G. Gibt es fossiele Tiefseeablagerungen von erdgeschichtlicher Bedeutung? Geol. Rundschau, 16, 1925, p. 435—468.

107. Stille, H. Alte und junge Saumtiefen. Nachr. Ges. Wiss. Göttingen, 1919 p. 337—372.

108. Stille, H. Die angebliche junge Vorwärtsbewegung im Timor-Ceram Bogen. Nachr. Ges. Wiss. Göttingen, 1920, p. 174—180.

109. Stille, H. Grundfragen der vergleichenden Tektonik, 1924.

110. Suess, E. Das Antlitz der Erde, 1888—1909.

111. Supan, A. Die Sundagräben. Petermanns Mitt., 53, 1907, p. 70—71.

112. Taber, S. The problem of the Bartlett Trough. Journ. of Geol., 39, 1931, p. 558—563.

113. Taber, S. The structure of the Bartlett Trough. Trans. Amer. Geoph. Un. Nat. Res. Council, 1932, p. 19—21.

114. Terra, H. de Structural features of gliding strata. Amer. Journ. Sc., 21, 1931, p. 204—213.

115. Tokuda, S. On the echelon structure of the Japanese Archipelagoes. Jap. Journ. Geol. Geogr., 5, 1926—'27, p. 41—76.

116. Tydeman, G. F. Hydrographic results of the Siboga Expedition. Siboga Expeditie, 3, 1913.

117. Umbgrove, J. H. F. Het Neogeen in den Indischen Archipel. Tijdschr. Ned. Aard. Gen., 49, 1932, p. 769—834.

118. Umbgrove, J. H. F. Verschillende typen van tertiaire geosynclinalen in den Indischen Archipel. Leidsche Geol. Med., 6, 1933, p. 33—43.

119. Umbgrove, J. H. F. In bibl. 120; Chapter VI, p. 140—162. The relation between geology and gravity field in the East Indian Archipelago; Chapter VII, p. 163—182. A short survey of theories on the origin of the East Indian Archipelago. 1934.

120. Vening Meinesz, F. A. Gravity Expeditions at sea 1923—1932, Vol. II, The interpretation of the results. Publication of the Netherlands Geodetic Commission, 1934.

121. Verbeek, R. D. M. Molukkenverslag. Jaarb. Mijnw. Ned. Indië, 1908, Wet. Ged.

122. Wanner, J. Geologie von West-Timor. Geol. Rundschau, 4, 1913, p. 136—150.

123. Wanner, J. Zur Tektonik der Molukken. Geol. Rundschau, 12, 1921, p. 155—165.

124. Wanner, J. Die Malaiische Geosynklinale im Mesozoikum. Verh. Geol. Mijnbk. Gen. Nederl. en Kol., Geol. Ser., 1925, p. 569—599.

125. Wanner, J. Mesozoicum. Leidsche Geol. Med., 5, 1931, p. 567—610.

126. Weber, M. Introduction et description de l'expédition. Siboga Expeditie, 1, 1902.

127. Wegener, A. Die Entstehung der Kontinente und Ozeane, 4th ed., 1929.

128. Willis, B. Isthmian links. Bull. Geol. Soc. Amer., 43, 1932, p. 917—952.

129. Wing Easton, N. Het ontstaan van den Maleischen Archipel, bezien in het licht van Wegener's Hypothesen. Tijdschr. Nederl. Aardr. Gen., 38, 1921, p. 484—512.

130. Wolff, F. v. Plutonismus und Vulkanismus. Handbuch der Geophysik, Bd. 3, Lief. 1, 1930, p. 32—348.

131. Wijkerslooth, P. de Bau und Entwicklung des Apennins (Dissert. Amsterdam), 1934.

132. Zwierzycki, J. Toelichting bij Blad XX der geologische overzichtskaart Nederl. Ind. Arch. Jaarb. Mijnw. Nederl. Indië, 1927, Verh. 1, p. 309—336.

LIST OF PUBLICATIONS CONCERNING THE SNELLIUS EXPEDITION (UNTIL 1934)

P. M. van Riel. Een Nederlandsche Oceanografische expeditie in den Oost-Indischen Archipel; de Zee, 1928, p. 292, 347.

P. M. van Riel. Die geplante Niederländische Expedition in die Meere des Ostindischen Archipels. Ergänzungsheft III der Zeitschrift der Gesellschaft für Erdkunde zu Berlin, 1928, S. 34.

J. Luymes. Korte schets van de ontwikkeling der Oceanographie en de expeditie van H.M. Willebrord Snellius. Tijdschrift van het Kon. Nederl. Aardrijksk. Genootschap. 1929, p. 337.

P. M. van Riel. The Netherlands Oceanographic Expedition in the East Indian Archipelago. Proceedings Fourth Pacific Science Congress. Java — 1929, p. 541.

J. Luymes. Short Sketch of the Progress of Oceanography and of the Expedition of the Royal Dutch vessel "Willebrord Snellius". Hydrographic Review 1930, p. 142.

P. M. van Riel. The Snellius Expedition. Nature. May 17, 1930.

E. van Everdingen. The Snellius Expedition. Journal du Conseil international pour l'exploration de la mer. Vol. 5, No. 3, 1930, p. 320.

Snellius Expedition. Verslagen. (Reports of progress). Tijdschrift van het Kon. Nederl. Aardrijksk. Genootschap, 1929, p. 530, 721; 1930, p. 29, 380, 708, 801, 991; 1931, p. 181.

Snellius Expedition. Verslagen. (Reports of progress by the Indian Committee for Scientific Researches). 1930, Eerste en tweede, 1931 derde Bulletin van de Willebrord Snellius-Expeditie.

P. M. van Riel. Ozeanographische Forschung in Niederländisch-Ostindien. Zeitschrift der Gesellschaft für Erdkunde zu Berlin, 1932, p. 208.

Ph. H. Kuenen. Die Viermeter-Lotröhre der "Snellius"-Expedition. Ann. d. Hydrogr. u. Mar. Meteor. 1932, p. 93.

P. M. van Riel. Einige ozeanographische Beobachtungen im Roten Meer, Golf von Aden und Indischen Ozean. Ann. d. Hydrogr. u. Mar. Meteor. 1932, p. 401.

P. M. van Riel. The Snellius Expedition. Journal du Conseil pour l'exploration de la mer. Vol. 7, No. 2, 1932, p. 212.

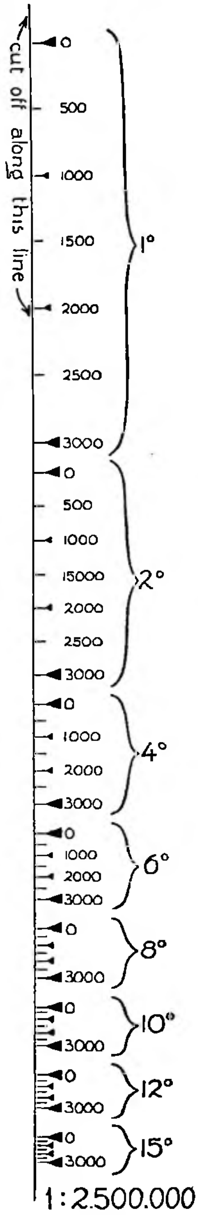
P. D. Trask. Origin and Environment of Source Sediments of Petroleum. American Petroleum Institute, 1932, p. 146—147.

H. C. Hamaker. Results obtained with a Thermograph for surface Temperatures during the Snellius Expedition. Journal du Conseil pour l'exploration de la mer. Vol. 8, No. 1, 1933, p. 64.

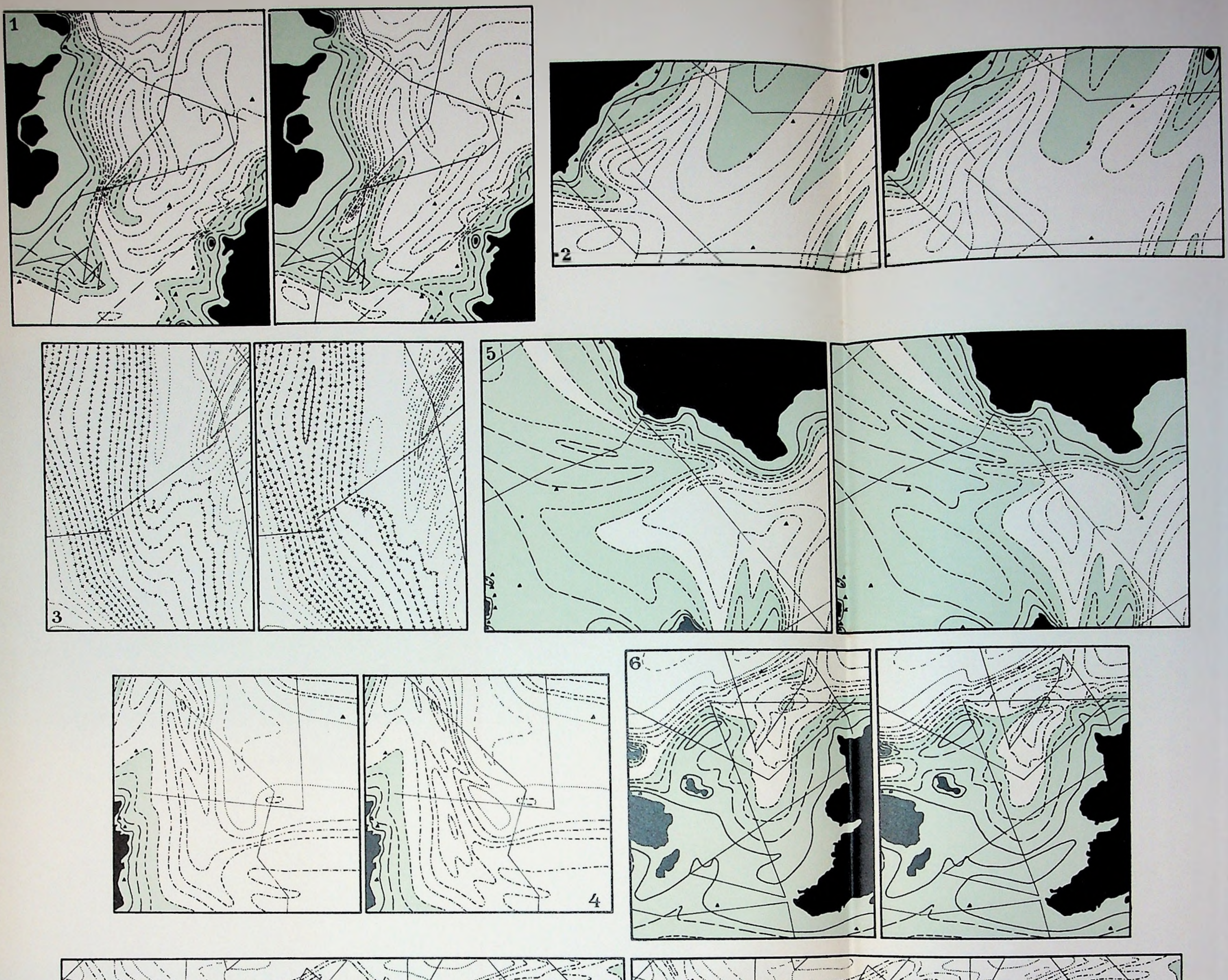
Ph. H. Kuenen. The formation of the atolls in the Toekang Besi-group by subsidence. Proc. Konkl. Akad. v. Wet. Amsterdam. 1933, p. 331.

Ph. H. Kuenen. Geology of Coral Reefs. The Snellius-Expedition, Vol. V, Part 2, 1933.

P. M. van Riel. The bottom configuration in relation to the flow of the bottom water. The Snellius-Expedition, Vol. II, Part 2, Chapter II, 1934.



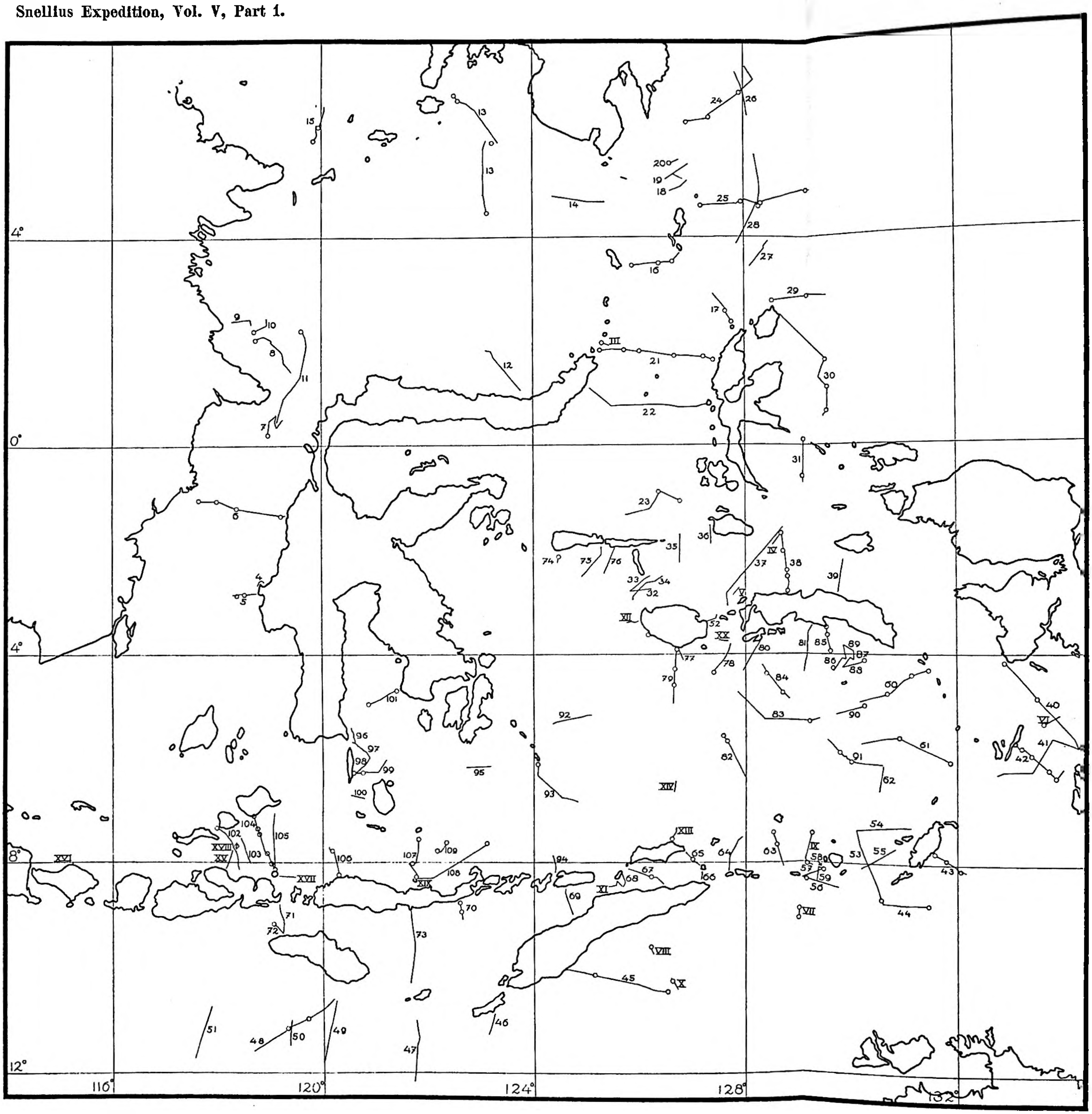
Declivity scale, for use on the large chart 1: 2.500.000. When cut off along the line indicated, this scale can be used for measuring the approximate slope of the sea floor.



Topografische Dienst 1934

Alternative constructions of the depth curves on the available data for several parts of the East Indies.
On the left original construction of the fairsheets.
Scale 1 : 2 500 000.

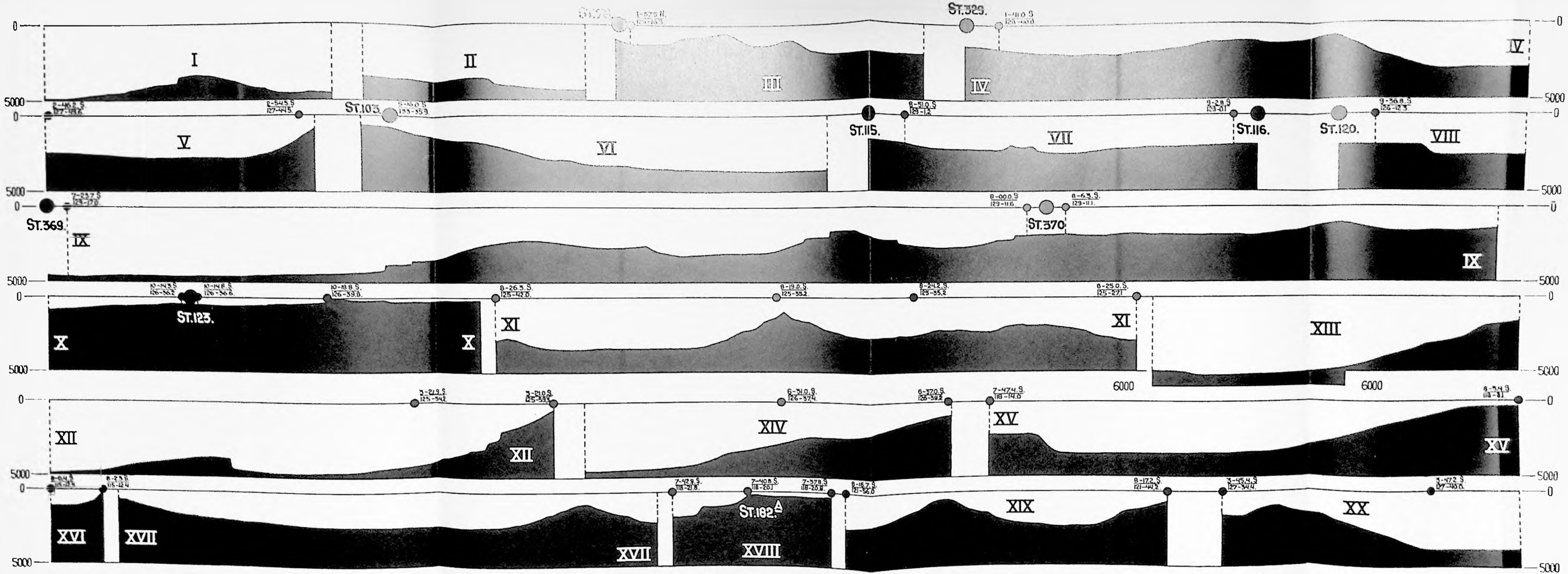
Foto-litho



Topografische Dienst 1934

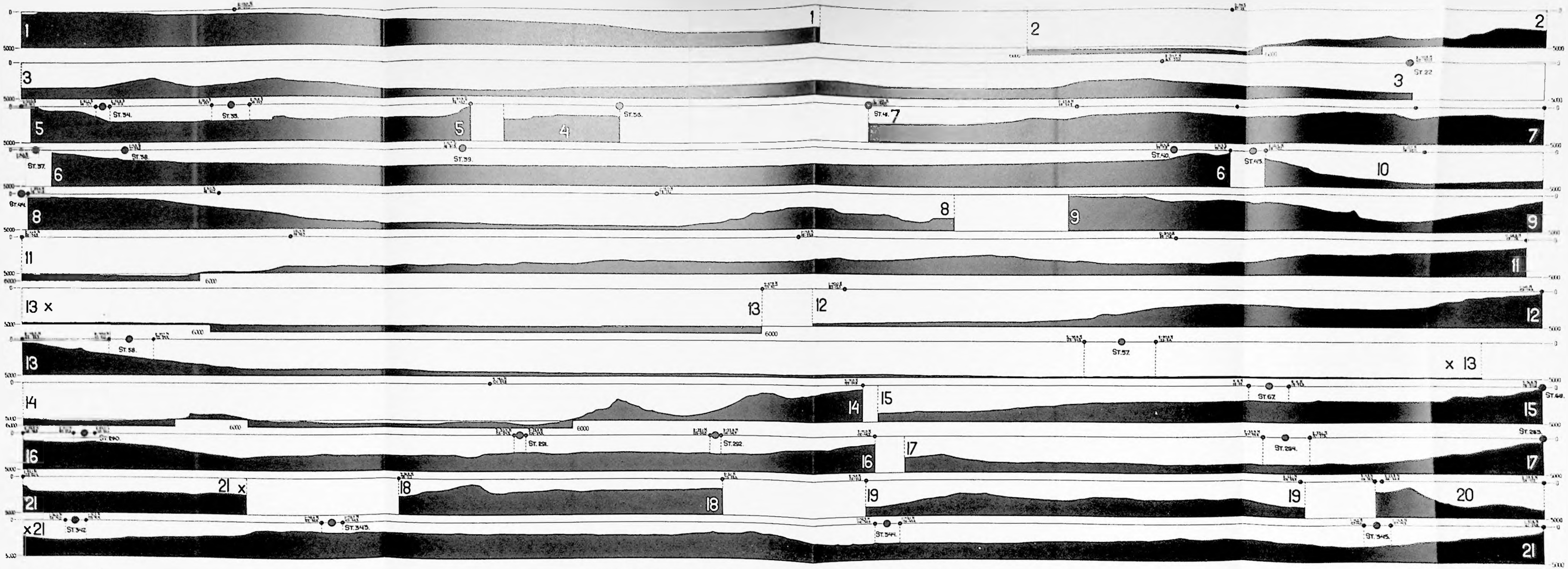
Map showing the position of the sections reproduced on Plates III—VIII.
Scale 1 : 9 000 000

Foto-litho



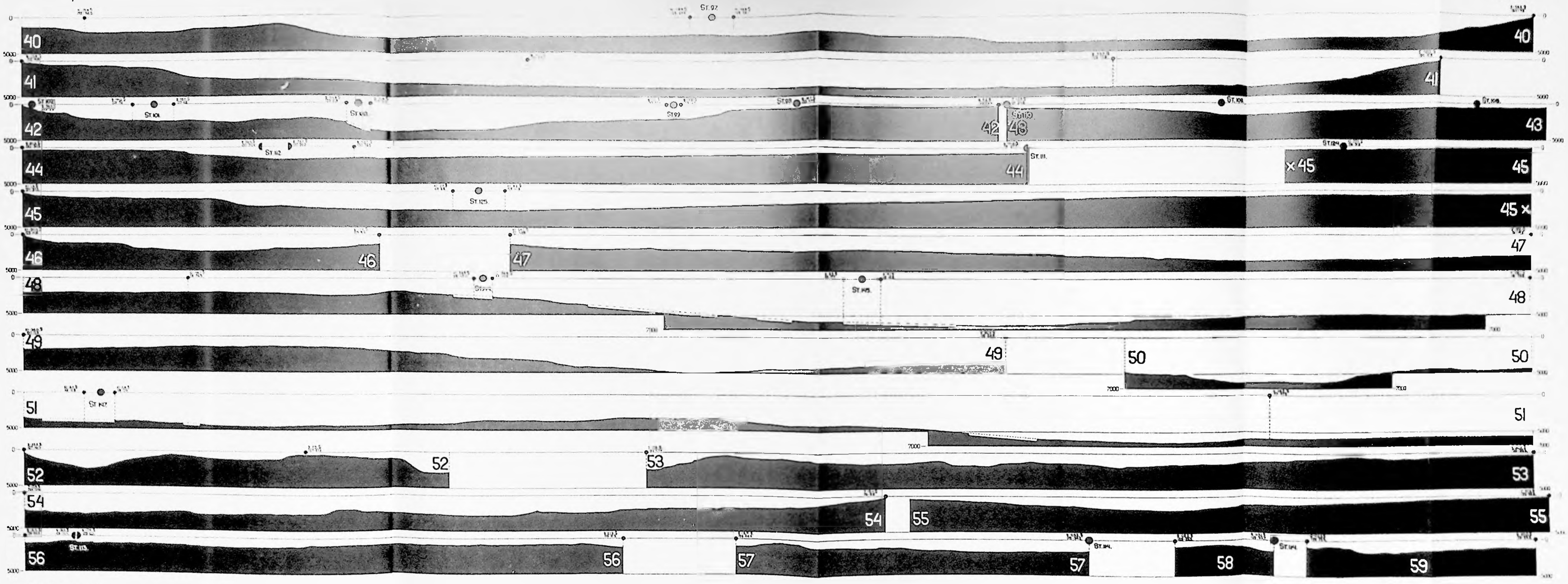
Sections of the sea bottom, redrawn from constructions based on echo-soundings, by F. Pinke, L. J. Veldman, T. H. Milo

Horizontal and vertical scales both 1 : 150 000.



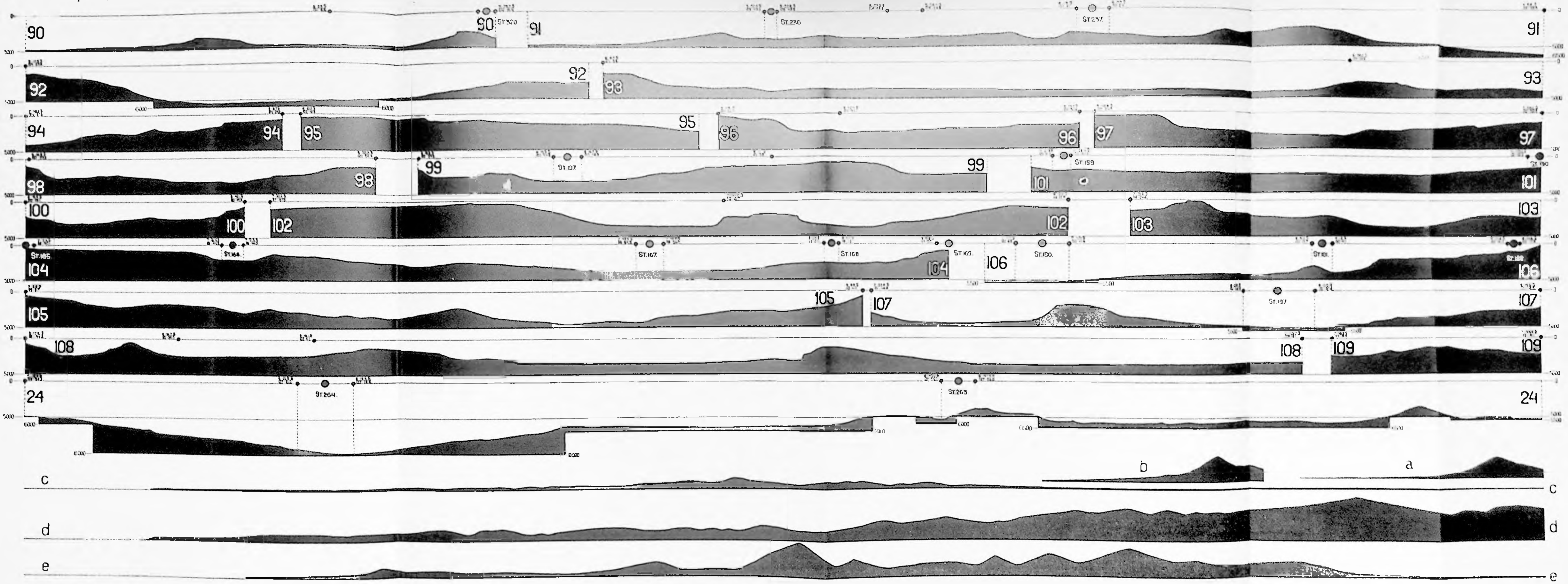
Sections of the sea bottom, redrawn from constructions based on echo-soundings, by F. Pinke, L. J. Veldman, T. H. Milo.
(1—3 Indian ocean, 4—7 Strait Makassar, 8—14 Celebes sea, 15 Sulu sea, 16—21 Molucca sea).
Horizontal and vertical scales both 1 : 300 000.


22
22 x
23
x22
25
26
27
28
29
30
30 x
31
32
33
34
35
36
37
38
39



Sections of the sea bottom, redrawn from constructions based on echo-soundings, by P. Pinke, L. J. Veldman, T. H. Milo (40—42 Arue basin, 43—46 Timor sea, 47—51 Indian ocean, 52 Strait Manipa, 53—59 around Babar).
Horizontal en vertical scales both 1 : 300 000.

Sections of the sea bottom, redrawn from constructions based on echo-soundings, by P. Pinke, L. J. Veldman, T. H. Milo.
(60—63 Weber deep, 69—73 Sawoe sea, 74—76 N.W.—Banda sea, 77—89 S.E.—Banda sea).
Horizontal and vertical scales both 1 : 300 000.



Sections of the sea bottom, redrawn from constructions based on echo-soundings, by F. Pinke, L. J. Veldman, T. H. Milo.
(90—93 Banda sea, 94—103 Flores sea, 24 Pacific ocean).
(Topographic sections: a. Tjareme volcano, b. Smeroe volcano, c. Prague-Breslau, d. Andes ± 27° S, e. Mont Blanc from NW to SE).
Horizontal and vertical scales both 1 : 400 000.

Topografische Dienst 1934

Foto-litho